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Roderer, John F., 2446 N. 6th
Rosenthal, Edwin, 517 Pine
Roussel, Albert E., 2112 Pine
Rudderow, Francis, 423 S. 15th
Rugh, J. Torrance, 1431 Walnut
Runkle, W. V., 1605 Christian
Ruoff, Wm., 1318 N. 6th

Sajous, Charles E., Paris, France
Salinger, J. L., 1510 N. 8th
Santee, E. I., 532 N. 6th
Schaffer, Charles, 1309 Arch
Schamberg, J. F., 831 N. Broad
Schneideman, T. B , 215 S. 17th
Schoales, Charles B., 1428 N. 11th
Schweinitz, George E de, 1401 Locust
Schwenk, P. N. K., 827 N. 7th
Scull, Wm. B., 3024 Richmond
Seiler, Carl, 316 S. 15th
Seiss, Ralph W., 213 S. 17th
Seltzer, C. J., 1501 Walnut
Seltzer, Chas. M., 608 N. 17th
Service, Charles A., City Line and Belmont av., Bala
Shea, Wm. Ker, 1705 N. 18th
Sheets, John, 1324 Spring Garden
Shellenberger, J. R., 5505 Main, Germantown
Shober, J. B., 112 S. 17th
Shoemaker, George E., 3727 Chestnut
Shoemaker, J. V., 1519 Walnut
Simes, J. H. C., 2033 Chestnut
Simsohn, Joseph S., 909 Franklin
Sinexon, Justus, 827 N. 20th
Sinkler, W., 1606 Walnut
Skidelsky, R. S., 514 Spruce
Skillern, P. G., 241 S. 13th
Skillern, Samuel R., 3509 Baring
Skilling, M. J., 1635 Christian
Slaughter, C. H. P., 742 Christian
Slocum, H. A., 1427 Walnut
Smith, A. D., 6067 Germantown av.
Smith, L. S, 133 S 18th
Smith, S. MacC., 1502 Walnut
Smock, L. P., 3330 Chestnut
Snively, I. Newton, 2501 Oxford
Somers, L. S., 3554 N. Broad
Sparks, Geo. W., 1022 Spruce
Spellissy, Jos. M., 108 S. 18th
Stahl, B. F., 1502 Arch
Steinbach, L. W., 1309 N. Broad
Stelwagon, H. W., 223 S. 17th
Stengel, Alfred, 332 S. 17th
Stern, Max J., 711 Franklin
Stevens, E. W., 1224 Walnut
Stewart, D. D., 108 S. 17th
Stewart, Wm. S., 1801 Arch
Stille, Alfred, 3900 Spruce
Stocker, A. E., 2212 Fitzwater
Stone, Edw. R., 1701 Master
Stone, James F., 1806 Green
Stout, E. J., 2422 N. Broad
Stout, Geo. C., 34 S. 18th
Stout, O., S. W. Corner 5th and Glenwood av.
Strawbridge, Geo., 202 S. 15th
Strittmatter, I. P., 999 N. 6th
Strobel, John, 948 N. 5th
Strouse, Fred. M., 2220 N. Broad
Styer, Charles, 1740 Columbia av.
Summers, S. Lewis, 309 E. Susquehanna av.

Talley, F. W., 1346 Spruce
Taylor, Charles F., 1520 Chestnut
Taylor, John J., 1520 Chestnut
Taylor, J. Madison, 1504 Pine
Taylor, Wm. J., 116 S. 18th
Taylor, Wm. L., 1340 N. 12th
Teller, Wm. H., 1934 Green
Thomas, Chas. H., 1633 Locust
Thomas, F. W., 6 Mount Airy av., Germantown
Thomas, George P., 2121 N. 7th
Thomson, Archibald G., 1426 Walnut
Thomson, Wm., 1126 Walnut
Thorington, Jas., 120 S. 18th
Thornton, E. Quin, 922 Spruce
Toboldt, A. L. A., 822 N. Broad
Tomlinson, H. A., Friends' Asylum, Frankford
Trautmann, B., 242 Franklin
Trojano, Giovanni, 733 S. 10th
Troth, Samuel N., 1339 N. 7th
Tull, M. G., 4807 Woodland av.
Tunis, Joseph, 129 S. 18th
Turnbull, C. S., 1719 Chestnut
Turner, John B., 1525 Christian
Tussey, A. E., 1208 Spruce
Tyson, James, 1506 Spruce
Tyson, T. Mellor, 1506 Spruce

Updegrove, Silas, 804 Marshall

Vanderslice, E. S., 127 S. 5th
Vandyke, Edward B., 306 S. 10th
Van Harlingen, A., 117 S. 18th
Vansant, E. L., 1929 Chestnut
Veasey, C. A., 47 N. 17th
Vogler, Geo. W., 565 N. 5th

Walk, Jas. W., 737 Corinthian av.
Walker, Gertrude A., 125 S. 16th
Walker, Jas. B., 1617 Green
Wallace, C. H., 4600 Kingsessing av.
Ward, E. Tillson, 843 S. 3d
Warder, C. B., 1212 N. Broad
Warder, Wm. H., 1401 N. 16th
Watson, Arthur W., 126 S. 18th
Watson, Edw. W., 131 N. 20th
Webb, Wm. H., 556 N. 16th
Weintraub, Sarah L., County Prison
Welch, W. M., 821 N. Broad
Wells, J. R., 5138 Lancaster av
Wells, P. F., 4023 Brown
Wells, W. H., 333 Pine
Werner, Marie B., 1514 Arch
West, John W., 1125 Wallace
Westcott, T. S., 108 N. 19th
Wetherill, H. M., 2208 Locust
Wharton, H. R., 1925 Spruce
Wheeler, E. B., 1922 Franklin
Whelen, Alfred, 1814 S. Rittenhouse Square
White, J. Willam, 1810 S. Rittenhouse Square
Whiteside, J. E., 6603 Haverford av.
Wightman, J. G., 2030 Wallace
Wiley, Eugene, 330 Reed
Wiley, Harry E., 330 Reed
Willard, DeF., 1601 Walnut
Williams, C. B., 1226 Spruce
Williams, Horace, 1717 Pine
Willits, Charles H., 24 S. 18th
Willits, I. P., 6123 Germantown av.
Willits, Mary, 1705 Mt. Vernon
Wilson, H. Aug., 1611 Spruce
Wilson, Jas. C., 1437 Walnut
Wilson, R., 1535 Pine
Wilson, Samuel M., 1517 Arch
Wilson, W. Reynolds, 112 S. 20th
Wirgman, Chas., 2021 Pine
Wise, George G., 420 S. Broad
Wolfe, Sam., 1701 Diamond
Wolff, Lawrence, 333 S. 12th
Wood, Alfred C., 214 S. 15th
Wood, H. C., 1925 Chestnut
Woodbury, Frank, 218 S. 16th
Woods, D. F., 1501 Spruce
Woods, Matthew, 1307 S. Broad
Woods, Walter V., 836 N. 41st

Yard, John L., 327 S. 18th
Yarrow, T. J., 1335 N. Broad
Yeager, Frank N., 2400 Oxford
Young, I. G., 1000 Shackamaxon
Young, J. K., 222 S. 16th

Zentmayer, Wm. J., 1423 Walnut
Ziegler, S. L., 1509 Walnut
Ziegler, Wm. H., 3028 Frankford av.
Ziegler, W. M. L., 1418 N. 17th
Zimmerman, N. W., 1633 Chestnut
Zuill, W. L., 857 N. Broad

HONORARY MEMBERS.

Browne, Lennox,	London, England.
Eskridge, J. T.,	Colorado Springs.
Kerr, John G.,	Canton, China.
Pavy, Frederick W.,	London, England.

DECEASED MEMBERS, 1896.

H. Ernest Goodman, M. D.,
James Graham, M. D.,
Arthur M. Hamilton, M. D.,
Harbeson Hickman, M. D.,
Wm. M. L. Richards, M. D.,
A. Sydney Roberts, M. D.,
Jas. W. Robinson, M. D.,
Wm. H. Stewart, M. D.

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THE IMPORTANCE OF THE SPECIFIC GRAVITY OF LIQUIDS FOR TOPICAL MEDICATION.

CARL SEILER, M.D.

[Read January 8, 1896.]

Ever since I published the first edition of my *Hand-book on the Throat*, in 1880, and even before that time and since, have I called attention in current medical literature to the importance of the specific gravity of liquids used as washes, douches, dressings, etc., for topical application.

But what little fruit has my preaching borne; how little attention is paid to this important factor by the general practitioner and by the gynecologist, rhinologist or surgeon, when he directs antiseptic dressings or washes, and lays great stress upon the decimal of his solution of mercuric chlorid, but carelessly prescribes salt and water to be used, or even designates the precise degree of temperature of the original douche and neglects the all-important physical factor, namely: The specific gravity of the solution, which, if not adequate, defeats the purpose of its use, namely to be curative.

The necessity of having a solution that is intended to be curative in its effects, and consequently is to be brought into intimate contact with the tissues and membranes of the body, of a density equal to that of the serum of the blood, or, to express it in more comprehensive terms, that there should be no interchange by osmosis between the cells or blood-vessels of the tissues on the one hand and the liquid in contact with the integument on the other, was forcibly brought home to me by an account in the daily *Press* detailing the damages to importers and packers of Spanish olives, by the Custom House officials.

In this account the statement was made, which on inquiry I found to be correct, that olives as consumed in this country as a table-luxury are packed and shipped in casks filled with a salt-solution of a certain specific gravity and that careful attention on the part of the shippers and packers is necessary to keep this salt-solution up to the exact standard of specific gravity, daily testing and a replenishing of every cask being required. A neglect of this precaution results, so I am told, and readily can understand by scientific induction, in either a shriveling of the fruit, if by evaporation the salt-solution becomes too dense, owing to the osmotic transfer of the water from the cortical cells of the olive into the surrounding medium, or in swelling and final bursting of the fruit, if by the soaking in of bilge-water into the casks the specific gravity of the brine is lowered and by the same osmotic action the excess of water saturates the fruit, which swells and ruptures through its skin and thus loses its market value.

To make my position still more secure, for I know that in bringing a subject such as this before the public at the time when apparently insignificant details in the healing art are entirely lost sight of in the search for the pathogenetic bacillus and the readings of the thermometer, when the old-fashioned indications of disease and the methods of combating it have been largely lost

sight of, and when the country practitioner, who, after all, makes up the rank and file of the profession, is at a loss to reconcile the readings of the thermometer, the definition of fever,* as it is, and the action of the innumerable synthetic antipyretics, I will call to mind the fact discovered and published by the late Dr. J. G. Richardson, in his investigations of dried blood-stains, that a solution of 56 grs. of salt (sodium chlorid) in one pint of water produces what is now known as the normal salt-solution, in which the blood-corpuscles become neither crenated nor globulated, thus enabling the microscopist to bring even dried blood-corpuscles back to their original size, and to measure them with an incredible degree of accuracy.

Unfortunately, the sciences of physics and chemistry, which are as fundamental to practical medicine as are physiology and anatomy, are sadly neglected in the studies of modern medical education, and the little that is learned by the student is either forgotten or neglected, or, still worse, misapplied, and for this reason I take up the cudgel against the cut-and-dried therapeutics supplied largely by the manufacturing chemists, and call attention to the infinite harm done by the unthinking negligence of the general practitioner who orders a topical application in the form of a solution by directing the patient to use so much water and a *little* of this, that, or the other thing, without regard to the specific gravity the resulting mixture will have.

It is a very common experience, in fact, a daily one, for patients to tell me that they have been instructed by their physicians to snuff up the nose some salt and water, but never, in my experience at least, has any of them been given directions as to how much salt and how much water. In the same way the gynecologist will bear me out in asserting that in like manner directions are given for using a hot vaginal douche with a little borax or soda, or carbolic acid, or what not, but never are directions given as to how much of the salts to how much water. Also compressed tablets of the more soluble salts, such as potassium or sodium bromid, potassium iodid, potassium chlorate, etc., are for the sake of convenience, ordered to be swallowed whole, without direction as to the amount of water to be taken with them. Finally, the surgeon, in his desire to prevent suppuration applies antiseptic solutions of mercuric chlorid or other germicide to the wounds during the operation and applies dressings of similar character afterwards, without regard to the specific gravity, wondering, no doubt, why the patient complains of pain when apparently no reason for such complaint exists.

That pain and congestion of the edges of wounds are produced by such solutions or dressings, I have repeatedly tested in my own person as well as in others, and also that a solution containing the same antiseptics in the same strength but made of the proper specific gravity, immediately subdues the pain and relieves the congestion.

Another well-known instance of the production of pain by improper specific gravity of liquids applied externally is the sense of burning and the congestion resulting from the application of undiluted glycerin to chapped skin of the arms or hands, and which immediately disappear when the glycerin is diluted to the proper density so that osmosis is prevented.

We all know that the mucous membranes of the body consist in the main of a thin layer of epithelial cells upon a basement membrane, immediately beneath which are the capillaries and the glandular structures, and in the case of erectile tissues, as in the nasal cavities and in the genito-urinary tract, the cavernous tissue is composed of a collection of venous sinuses. In open wounds the different vessels and lymph-spaces are separated from the external air by but very thin membranes, or to be more exact, aggregations of cells forming a membranous envelop for their contents. In both instances the terminal sensory nerve-filaments are in close proximity to these membranes.

An arrangement such as this presents the most favorable condition for osmotic action through this membranous envelop if a liquid of greater or less

*Dunglison.

density than that contained within the membranes is brought in close contact with them on the outside, and as in the case of the olives (which fruit, by the way, in its structure closely resembles the cavernous tissues of the human body) the cavities become surcharged with liquid, if the fluid used as a wash or dressing is of a less density, thereby causing swelling and pressure upon the nerve-filaments, and consequently, in the first instance pain, and as a secondary effect, owing partly to the irritation of the nerves and partly to the engorgement of the capillaries, congestion of the parts so that the object of the dressing, wash, or douche is defeated. If on the other hand the liquid used is of a greater density or specific gravity than it should be, the watery elements of the juices will ooze out of the channels and cavities of the tissues by exosmotic action through the membranous envelop, and again as in the case of the olive, a shriveling of the membranes will take place, together with an abnormal accumulation of the solid elements within the cavities and channels, and again pain as well as congestion, will be the result. Of course I now speak only of such topical applications as do not contain substances in themselves irritating to the nerve-filaments, irrespective of the specific gravity of the solution. In many of the so-called antiseptic solutions the ethereal oils of various plants, which are exceedingly irritating and may, if used alone and uncombined, be classed among the vesicants, are not properly incorporated in the solution, so that by virtue of their oily nature they float on top of the liquid and thus come in direct contact with the tissues or membranes, acting as powerful local irritants. It will be seen, therefore, that it is of the greatest importance to have an exact specific gravity for our liquid topical applications, whether they be in the form of spray, wash, douche, or dressing, and to be as exact as possible in our directions to the patient for the preparation of such application to be used by him. It is not sufficient to tell him to sniff salt and water up his nose; nor is it sufficient to give such vague directions as a teaspoonful of salt in a pint of water, because teaspoons vary in size, and may be heaped with salt or made not quite full, and a pint in the minds of most persons is a variable quantity, because, as a rule, a so-called pint-bottle is used as a measure, and as we all know, pint-bottles vary in size; and finally it is frequently the experience of the rhinologist as well as the gynecologist, that such topical applications are made by the patient in a hurry, and he or she but rarely allows sufficient time for the salts to dissolve completely before making use of the application; so that in spite of careful directions the desired result is not obtained.

Long experience has taught me the advisability of prescribing a concentrated solution carefully prepared by the druggist, a sufficient quantity of which the patient is directed to add to the exact quantity of water, so as to obtain not only a perfect solution but also one of the proper specific gravity.

In most instances neutral unirritating sodium chlorid is the best agent with which to obtain the desired specific gravity—by using 56 grs. of the salt to a pint of water, to which there may be added the other ingredients as desired. If, however, alkalies, such as sodium bicarbonate or sodium biborate, or both, are desirable in a wash or douche for the purpose of rapidly dissolving the viscid mucus covering the mucous membranes in catarrhal conditions—and in that case a considerable quantity of these alkaline salts is required—it is best to make the alkaline solution first of the desired strength and then bring it up to the required standard of density by the subsequent addition of sodium chlorid.

DISCUSSION.

Dr. John C. Da Costa took exception to the statement made by Dr. Seiler, that it is the practice among gynecologists to prescribe an indefinite quantity of a salt, or of boric acid and the like, to be dissolved in an indefinite quantity of water, to be used for a vaginal douche. On the contrary, it is the custom to order a definite quantity of the salt to be dissolved in a measured pint, or two pints, of water. For instance, a teaspoonful of salt in a pint of water makes very nearly the normal salt-solution which has been referred to. As for other solutions to be used for wounds, it is a question whether or not the specific gravity has as much to do with the resulting irritation and the like, as the character of the solution. Thus, a solution of mercuric chlorid, 1-2000, has produced erythema and considerable local irritation; whereas ordinary salt-solution of this strength would cause no irritation. Another illustration that it is not the specific gravity of liquids that is of the most importance, is that of distilled water, which is not irritating and is one of the best dressings for wounds. Glycerin is irritating, not on account of its specific gravity, but because of its great affinity for water; and failing to get water from *dry* surfaces, attracts it from the deeper tissues.

Dr. Seiler admitted that he had not had any experience with gynecologic cases; but he could recall many instances in the country in which just such indefinite instructions were given as he had mentioned in his paper. With regard to the statement concerning glycerin, it may be said that it attracts water on account of its increased density, which makes a demand for moisture from the tissues. Glycerin in the air does not attract water, and applied to the dry skin it will not cause irritation; but if there are cracks in the skin it will draw water from the blood-vessels by osmotic action.

Dr. John C. DaCosta related, in regard to the hygroscopic character of glycerin, that a certain druggist had the reputation of taking the heads out of his barrels of glycerin on damp days in order to increase the quantity.

CALCULOUS PYELITIS.

ALOYSIUS O. J. KELLY, M.D.

[Read January 8, 1896.]

It is because of several interesting and rather unusual clinical and pathologic features that the following case is accorded the dignity of a report. The patient, C. F., who was seen for the first time November 9, 1895, was a female, aged fifty-six years, of German extraction, twice a widow, and a housekeeper by occupation. She was the mother of five children, three of whom are still living and well. One, a male, died at the age of nine years from a cause unknown; the other, a female, at the age of twenty-five years, of insanity. Her father died of pulmonary tuberculosis at an unknown age; her mother of apoplexy at the age of sixty years. The patient had always enjoyed good health until about one year prior to her death, when she began to complain of lassitude, and had headache and backache; her appetite became poor and she was otherwise dyspeptic. Two months before she came under observation she noticed in her abdomen a swelling which occupied particularly the right upper quadrant. When seen for the first time, the patient complained of loss of appetite, some nausea and vomiting, and insomnia, and was aware of the presence of the tumor, which, however, caused no discomfort, nor since discovery had it appreciably increased in size. She never had any pain. There was no headache or vertigo, nor were there any symptoms referable to the thoracic viscera. Flatulence and constipation were absent. There were no urinary symptoms. The menopause had been successfully passed thirteen years previously.

The patient was very stout, rather markedly anemic, the skin dry, the pulse somewhat accelerated, and the temperature slightly elevated. There was no edema, and the intellect was unimpaired. Examination of the thoracic viscera disclosed only a slight accentuation of the second aortic sound. In the abdomen, on the right side just below the liver, there was plainly noticeable and readily demonstrable a tumor about the size of a large cocoanut. The thickness of the abdominal wall interfered with satisfactory percussion and other examination.

The tumor did not move with the respiratory excursions. The urine was rather pale-yellow in color, turbid, of a specific gravity of 1016, neutral in reaction, and contained albumin. Microscopic examination after centrifugation revealed quantities of pus-cells, mucus, and epithelium, some of which apparently partook of the nature of that ascribed to the pelvis of the kidney, but most of which was in an advanced state of fatty degeneration—mere *debris*. The urine contained no sugar, nor were casts or concretions of uric acid, urates, oxalates, or phosphates, etc., discovered.

The diagnosis of pyelitis or pyonephrosis was made, and the treatment pursued was directed chiefly to the relief of vomiting, which was moderate, and the condition of the urine. No real improvement ensued. The condition of

the urine and the other symptoms continued practically unchanged, and to them were later added more marked evidences of uremia—headache, stupor, coma. In this condition the patient remained for twenty-four hours, and died November 19, 1895.

The autopsy was performed about twenty-four hours after death. The body was that of a well-developed, well-nourished female, with an exceeding amount of adipose tissue. There was no edema; post-mortem lividity and post-mortem rigidity were marked. The lungs, excepting a minor grade of emphysema, were normal. The left ventricle of the heart was slightly hypertrophic; the mitral, aortic, tricuspid, and pulmonary valve-leaflets, apparently normal. The aorta exhibited slight atheroma. The liver, spleen, pancreas, and gastro-intestinal tract were normal. Embedded in a great quantity of perirenal fat, from which it was difficult to separate it, the right kidney was found much enlarged, the enlargement being due to the presence of a quantity of fluid. During the extraction of the kidney a small puncture was accidentally made in a greatly atrophied portion of the organ near the summit, and a large quantity (estimated at about a pint and a half) of yellowish sero-purulent fluid and thick grayish-yellow, crumbling, grumous, or putty-like flakes escaped. Incising the kidney, a large stone was encountered occluding the ureteral outlet. The pelves and calices were immensely dilated, especially in the upper part of the kidney. The renal substance was much decreased in thickness, particularly in the region where the puncture mentioned was accidentally made; here it was not more than an eighth of an inch thick. The organ was in reality transformed into a veritable many-chambered cyst, the various compartments of which opened into the dilated pelvis. The dilated calices were filled with flakes similar to those already noted. When these were washed away the mucous membrane bore a resemblance very much akin to the flakes, being dull, grayish-yellow, opaque, thickened and eroded. The atrophy of the kidney was in places so extensive as to be almost complete, the proper renal substance being apparently replaced by a connective-tissue envelop.

The left kidney also was embedded in a great quantity of fat, and was about normal in size. On section about two ounces of thick, creamy, greenish-yellow pus escaped, and two stones lodged in the pelvis and extending into the calices were encountered. Washing away the pus, no flakes similar to those found in the right kidney were present, but the mucous membrane of the pelvis partook of the same characteristics as did that of its fellow. The proper renal structure was much atrophied and was in places difficult of separate recognition. The adrenals exhibited no abnormality. The ureters were dilated above, and their mucous membrane turbid and lusterless. The bladder contained a small quantity of cloudy urine, but no calculus. The bladder-wall was not particularly altered. The genitalia were apparently normal.

A later microscopic examination of the grumous flakes revealed them to be composed of degenerated epithelium, pus-cells, and indeterminate *debris*.

The largest stone, the one lodged in the right kidney, weighs 7.7 grams. It is V-shaped, hard, and externally is of reddish-brown or brownish-black color. On section it is of a paler or dirty-brown color and presents an appearance akin to that of cancellous bone. Of the two calculi from the left kidney, the larger weighs 5.6 grams, the smaller 3.1 grams. The smaller, of brownish-black color, is wedge-shaped with a lateral protuberance. On section it is light-brown in color and distinctly lamellated. The larger one, in color between a dirty-gray and reddish- or brownish-black, is somewhat coral-shaped, indicating extension into the various calices. The three calculi are composed of urates, especially of ammonium, ammonio-magnesium phosphates, basic magnesium phosphate, and neutral calcium phosphate. The urates, while distributed to some extent throughout the calculi, are more particularly confined to the surface. No nucleus of any different formation was discovered.

The case is thought interesting from

several points of view—in its clinical bearing, in its pathologic bearing, and in its therapeutic bearing. Reasoning from the pathologic to the clinical, it is really remarkable that a patient with such extensive organic destructive disease of the kidneys should for so long a time be devoid of any symptomatic indication. The few and trivial manifestations of pathologic processes presented by the patient during the last year of her life were so slight as to occasion no discomfort or concern and to lead to the employment of no measures for their betterment. It was not until the accidental discovery of the abdominal mass that her condition was thought to call for medical attention. At this time the renal condition can have been but slightly different from that found post-mortem. The presence of calculi was not considered probable, as no symptoms whatever of renal stone were present, save the pyelitis. The causative relationship that the former, however, holds to the latter was of course borne in mind, but otherwise there existed no symptoms upon which to base such a diagnosis. The clinical fact of renal as well as other calculous formations being often present in the body, without occasioning distinctive symptoms, is well known. The extensive destructive disease of the kidneys steadily progressing without symptoms until the increasing involvement of the renal parenchyma, occasioned manifestations of uremia, is however, rather unusual and extremely interesting.

From a diagnostic standpoint, but also bearing upon treatment, it is of service to recall the recent communication of Howard Kelly, upon the "Diagnosis of Renal Calculus in Women"[1] as well as previous communications upon disease of related organs.[2] Kelly not only directs our attention to the long acknowledged serviceable catheterization of the ureters, but adds other refinements of diagnosis in the nature of renal catheters and bougies, by means of which a direct examination of the kidneys is accomplished and unmistakable and positive signs of renal calculus may be elicited. In the case now reported, the presence of the tumor on the right side, in conjunction with the other evidences of pyelitis, rendered certain that the right kidney was affected, but established no criterion as to the health of the left one. Catheterization of the ureters and direct examination of the kidneys as suggested by Kelly would probably have furnished a basis for diagnosticating the calculous form of pyelitis, and would certainly have rendered evident the bilateral nature of the affection.

It is this latter fact, the involvement of both kidneys, that lends an added interest to the case. Renal calculi and their sequelæ, pyelitis and other affections, are usually unilateral. In considering the question of surgical treatment it is essential to determine the condition of both kidneys, and one may not assume, as is too frequently done, that such disease of one kidney presupposes an intact fellow—a question of easy determination in women. It is in this connection that the suggestions of Kelly are of great import, for in such cases, while we may not be able to effect a cure either by surgical means or otherwise, it may often be possible by means of renal catheterization, to produce at least temporary amelioration of symptoms and prolongation of life by renal medication.

The case was observed in the private practice of my father, Dr. J. V. Kelly.

1 *Medical News*, November 30, 1895.

2 *Johns Hopkins Hospital Bulletin*, Nov., 1893; Feb., 1895; *American Journal of the Medical Sciences*, Jan., 1894.

DISCUSSION.

Dr. J. P. Crozer Griffith commented upon the fact that in the case reported the two kidneys were affected. This in its turn suggests caution as to possible surgical interference, which might have been undertaken in the case. This patient was living, until within a short time before death, in comparative comfort. If a tumor had been found upon one side of the abdomen and pus in the urine, coming from the kidney, and had doubt existed whether an abscess had opened into the kidney or pyonephrosis existed, an experimental incision might have been made and the kidney removed. The death of the patient would at once have followed. Cystic kidney is common as a congenital condition and generally is bilateral. Dr. Griffith referred to two cases in which a cystic kidney was removed, the operator not knowing that the other kidney was diseased, and the patients died.

The diagnosis of these diseases of the kidney is involved in great difficulty. For instance, the disease may begin as a nephritis, then pass into pyelonephritis, and finally into pyelonephrosis. Finally, calculus may form and complicate the case still further. The question then arises whether the calculus is located in the kidney or in the bladder. When in the bladder, vesical symptoms are likely to be present—frequent urination, pus in the urine, which is of alkaline reaction. The existence of acid urine leads to the conclusion that the kidney is the site of the disease, and not the bladder; but in some cases the urine may be acid and contain pus, and the introduction of a catheter show that the urine coming from the kidneys is healthy, and that from the bladder contains pus. In the case of a boy ten years of age, in which the diagnosis rested for a long while between Pott's disease and calculus of the kidney, the pain in the back, especially upon movement, led to the suspicion of Pott's disease; but the presence of pus in the urine, with uric-acid crystals, established the correct diagnosis and treatment. The result was entire recovery, with disappearance of the uric acid.

The diagnosis between pyonephrosis and movable kidney is sometimes difficult. When pus is found in the urine with crystals of calculus, the diagnosis is plain; but sometimes there are no such conditions of the urine present and crises of pain may occur just as in movable kidney, making the diagnosis difficult.

Dr. A. J. Downes related a case that had been under observation for three years, in which the amount of pus in the urine was very great. The patient had had renal colic and had also passed stones. The urine was very turbid. The man had a pyonephrosis and stone probably in the left kidney and was consulting on the advisability of having an operation performed. He was advised to have a preliminary supra-pubic cystotomy performed, under local anesthesia if possible, in order to find out the condition of the right kidney before attempting an operation on the diseased kidney. The man had a large family, and was in bad condition for operation, and it was feared that he might not take the anesthetic well on account of the condition of the kidneys. The urine was very turbid, and differential diagnosis with the aid of the cystoscope, as to what was emitted by each ureter, was impossible, as in a few minutes after washing out the bladder its contents became opaque from mixture with the incoming urine. Preliminary cystotomy was proposed in order to have the opportunity, as in the female, of catheterizing the ureters to determine the condition of each kidney. The specific gravity of the urine was 1010, the quantity from ninety to one hundred ounces in twenty-four hours; but the man had been drinking large quantities of lithia-water for some time and he was passing water freely as the result. It might be very dangerous to operate upon the left kidney in this case without knowing the condition of the other.

Dr. Howard S. Anders related a case treated conjointly with Dr. Willard, in which a man who had a pronounced uric acid diathesis, presented evidences of bladder-irritation and pus in the urine. On passing a sound a clicking was heard, giving evidence of stone in the bladder. Dr. Willard advised supra-pubic cystotomy, and this was done; a small vesical calculus was found, of irregular form and dark color, resembling a grain of giant gunpowder. After the incision healed, the urine continued purulent. The reaction of the urine had been acid all the time, with signs of bladder-irritation. The pus in the half-gallon bottle, which was used to collect the urine, formed a layer from a half to one centimeter in depth. The specific gravity was 1018, and the supernatant urine was tolerably clear. One morning, about a month after the supra-pubic incision had healed, Dr. Anders was sent for in haste. The patient had come down stairs to the library, grew very pale and faint, and in a short time died. The question came up as to the cause of death. The autopsy revealed pyonephrosis, with a large pus-sac containing numerous uric-acid calculi. The pus-sac contained a thick pyogenic membrane continuous with that in the pelvis of the kidney. In the heart the coronary arteries were infiltrated with calcareous deposit and the right coronary orifice was also calcareous and exceedingly small. This vascular condition had been the cause of sudden death. The left kidney presented a condition of pyonephrosis, with numerous granular, dark, uratic calculi. The case is interesting

from the fact that during life there had been no evidence of such marked renal involvement, except the persistence of pyuria after the supra-pubic cystotomy, and after all causes of bladder-irritation had been removed.

Dr. F. S. Pearce stated that in some cases of pyonephrosis the opposite kidney is wanting; *i. e.*, there may be only a single kidney present. He related the case of a woman who died of erysipelas, at the autopsy only one kidney and that badly riddled with pus-sacs being found; almost all the structure of the kidney was gone, and the remains were filled with small calculi and scales of uric acid. If, in this case, an operation had been performed, the patient would have perished. In another case in which litholapaxy had been performed, and later, to relieve symptoms, supra-pubic cystotomy, with washing out of the bladder for several months, the patient finally died with uremic symptoms. In this case it was thought that the operation had made the bladder smaller and the contraction had forced the debris back into the kidney, causing more rapid double pyonephrosis, as found at autopsy. Certainly the bladder was very small. In either of the cases cited an operation would have been dangerous and proved ultimately fatal in one.

Dr. Downes did not think that it ought to be difficult to diagnosticate between pus in the bladder and pus in the kidney. If the bladder be washed out thoroughly with dilute hydrogen dioxid and then flushed out clean with hot sterile water until the fluid returns perfectly clear, by waiting fifteen or twenty minutes sufficient urine, as it comes directly from the kidney can be secured for examination.

Dr. Thos. S. K. Morton stated that the ureters of the male have been catheterized. One of the surgeons connected with the Johns Hopkins Hospital had performed the operation six or seven times, and one or two others are reported as having accomplished the feat. It is said to be very difficult. It has been stated that Dr. Nitze has modified his ureteroscope so that by its aid the male ureter may be catheterized. Whether it would be easy or difficult to catheterize the ureters after a supra-pubic cystotomy, it would be hard to say, but from the trouble encountered in intravesical manipulations in some cases, it is likely that it would be very difficult.

Dr. Kelly added that he considered the case especially interesting because of the latency of the symptoms until such a short time prior to the patient's death, and that the bilateral character of the lesion suggests *festina lente* in thinking of surgical interference in such cases.

FOUR CASES OF CONGENITAL NEVI.

C. D. SPIVAK, M.D.

[Read January 22, 1896.]

He stated that he had presented the cases with the object not so much of instructing the members, but rather of being instructed by them. They represented various forms of vascular nevi. Dr. Spivak did not attempt to go into the pathology or the etiology of the cases, but presented them for clinical study.

The first case, a child three months old, had a single nevus upon the forehead. When first noticed it was the size of a pea, and was located at the root of the nose on the right side; but it had increased so as to extend over the bridge of the nose and also involve the whole structure of both eyebrows, causing marked disfigurement.

The next patient was eleven months old, and the lesion existed on the right side of the chest. When first seen it was not one-fourth its present size. It had increased in thickness as well as in area. One would suppose that these nevi would increase in the same ratio as the general growth of the body, but in this case the increase had been more rapid than the development of the child.

The third case was one of multiple nevi in a child nine months of age. There were several small aneurismal growths upon the scalp and one on each temple. The latter were diminishing in size, in evidence of the fact that these growths may spontaneously disappear. There were a number of them upon the body; *e. g.*, five upon his back and one upon the abdomen. One was upon the right palm, and in process of disappearing; and one was on the big toe, and was also disappearing.

The last case, in an infant eight months of age, exhibited a peculiar congenital growth upon the scalp, which appeared to be a nevus. It was of lighter color than the rest, firmer in texture and distinctly lobulated. The mother said that it never swelled up and had always been the same size.

In none of these cases was there any element of maternal impression or anything of the kind, so far as could be ascertained.

DISCUSSION.

Dr. Ernest Laplace stated that nevi are always interesting and that the cases presented were good examples of the different forms. The word nevus is perhaps not the best one, or not as correct a term etiologically or histologically to describe the condition as angioma. The cases presented were really illustrations of angiomata, of tumors consisting of blood-vessels exclusively. Microscopically, the tumors consist largely of capillaries, and generally more venous than arterial. They are made up of vessels that have only two coats, lacking the muscular tunic or only possessing traces of it. They are fibrous tubes, with traces of cellular tissue. There is nothing extraordinary about an angioma of such character, except that it is a vast net-work of what would be otherwise the capillaries of the skin. The growth extends between the layers of the skin, without following the blood-vessels through to the papillæ beneath. If such a formation were cut through one would still be above the deeper layers of the cutaneous epithelium. At the red border of the growth the four layers of skin are present and show the vascular network. For some reason or other, either irritative or otherwise, these blood-vessels on the surface of the body have grown out of proportion to the surrounding structures of the skin. There being no epithelial layer to confine the blood-vessels and prevent them from growing, the only treatment to adopt is by some means to prevent their further growth—such as punc-

ture with the thermo-cautery, in order to cause reaction, the object being to convert the mass into fibrous tissue. This is done in several sittings, as much being accomplished at one time as the patient can stand. In the case of a little patient, more than 120 punctures were made in the course of twelve sittings.

The fourth case presented partakes of the nature of a papilloma; but the growth is not composed of the papillary layer of the skin exclusively, and it might be called an angio-papilloma. It probably consists of large blood-vessels growing as a papillary tumor. If the child lives the tumor will also continue to develop. It has a nutrient artery of its own and the papillary character of the growth is becoming obscured and it is spreading by increase in the vascular elements mainly.

Dr. Carl Seiler stated that there had been presented three different varieties of birth-mark or nevus. The fourth case he considered a different kind of tumor, that is to say, an angiomatous papilloma. The others appeared to be true angiomas. Whether a nevus develops as the result of irritation of the skin or of atmospheric pressure, or what not, he was not prepared to say, but a true spongy, cavernous-tissue nevus does grow ; as there is absolutely no pulsation, however, in the first case, the formation is an ordinary angioma constituted of venous sinuses, there being no difference between these vessels and venules elsewhere in the body. On the other hand, in the cases in which the tumor consists of arterioles and capillaries as well as venules, the same influence that causes the others to grow, *i. e.*, want of pressure, causes these to shrink.

As regards treatment, it was agreed that galvano-puncture is the best, except in instances in which the growth is of such character that its base may be readily encircled by a well-waxed cord and strangulated.

Dr. Harrison Allen thought electrolysis the best treatment in all cases. He reported the case of a child-in-arms, probably not above fourteen months old, that was brought to him about a year before with a nevus, the size of a pea, at the end of the nose. It was a venous swelling, congenital in origin, and was increasing in size, so much so that a physician who had had charge of the case in a neighboring State, had attempted ligation. A distinguished surgeon had also been consulted, and advised ablation of the whole mass and making a new nose by a flap from the forehead. Dr. Allen told the parents that he thought something could be done for the child by electrolysis. He had made for the purpose a positive needle which was covered with shellac to within about one-third of an inch of the free end. This needle was pushed into the cheek through the skin at a point outside of the limit of the growth. Only enough current was passed to whiten the tissue. The first application was very cautiously made, the next less so and the others with perfect freedom. The tumor could be seen to shrink under the treatment until it disappeared. If the needle had been thrust into the growth directly, there would have been a furious hemorrhage.

HALF OF THE LOWER JAW REMOVED FOR EPITHELIOMA.

W. JOSEPH HEARN, M.D.

[Read January 22, 1896.]

I wish to present this specimen as an example of the sad results following delay in the proper removal of an epithelioma of the lower lip. The patient was a male, aged sixty years, who presented himself at the surgical clinic of Jefferson Medical College Hospital on November 6, 1895, suffering with a large tumor on the lower jaw, which he said had been called an osteoma. A slight scar on the lower lip attracted attention to the fact that a small ulcer had been removed by the knife one year previously. On examination of the sub-mental and sub-maxillary regions, enlarged glands were found. The growth was a secondary carcinoma with so much of the overlying skin, as well as the masseter muscle involved, that the only operation for relief was the removal of half of the lower jaw, as well as all the enlarged glands in the surrounding structures. The operation was completed with the loss of less than four ounces of blood, and was much facilitated by dividing the lower jaw in almost the first stage of the procedure. The angle of the lips was not divided. Most of the vessels were tied before their division—hence the small loss of blood. The patient made a rapid recovery, as all cases of operation about the face generally do. An extensive dissection of the skin-flaps was necessary in order to make them meet easily. No tension was permitted anywhere along the line of sutures.

Among the laity, and even among many of the profession, the term carcinoma and malignancy are synonymous. That is not, strictly speaking, true of carcinoma of the lower lip. Thorough and prompt removal will, in the majority of cases, lead to a complete cure. Many cases of my own, operated upon fifteen years ago, are still under observation.

It was not the purpose of this paper to consider the diagnosis or pathology of epithelioma of the muco-cutaneous structures, but I wish to express the opinion that any practitioner allowing an ulcer, fissure, or nodule of the lip to remain uncured in a person over thirty years of age, and who does not operate or have it removed, incurs a responsibility that is almost criminal—providing, of course, the patient is willing to submit to treatment and to follow advice. Not only must the new growth, with a large portion of the adjacent structures, be removed, but also the submental and sub-maxillary lymphatic glands and ducts as well. This may seem radical, but this practice is taught by the foremost surgical teachers of this country. If followed, it may save a life. Snow has found cells of carcinoma in the marrow of the arm within six weeks after carcinoma of the mammary gland had been discovered. Hence the necessity of early operation is evident. In all these operations the golden rule is to excise dangerous lymph-glands in the definite infective path before a late stage of their enlargement is reached.

When operative intervention is impossible on account of the extensive inroads of the disease, or if the condition of the

patient forbids it, the treatment should consist in making the patient comfortable. In the London Cancer Hospital the patients are, under such circumstances, allowed to form the opium-habit without restraint. It acts by preventing the proliferation of the carcinoma-cells. Opium exerts a powerful inhibitory action upon the growth of carcinoma-parenchyma, materially checking cell-proliferation. The neoplasm kills not so much by growth at the primary site as by infective metastases from this area. We all know how rapidly secondary carcinoma develops in the lower jaw. When confined to the lip its source of blood-supply is limited to one side, but when the glands are involved the blood is supplied from all sides, and hence their rapid growth. The form of opium I prefer is acetum opii. This is less constipating and agrees better with the stomach than other opiates. Local treatment of carcinoma is useless, although in cases of doubt—as when there is ulceration or fissure—I employ a favorite ointment having the following composition:

℞ Balsam. Peruviani
Unguent. Hydrargyri Nitratis . . ää ʒi.
Petrolati . . . . . ʒi.
M.

If healing does not take place in two weeks, I operate at once. Carcinoma never heals in this way. In cases of open carcinoma of the lips, the enlargement of glands may be due to secondary infection, just as may happen with any other open ulcer; but when there exist hard nodules, without ulceration and with enlarged lymphatic glands, there need be no delay, as the disease is carcinoma and nothing else. It is not within the scope of this paper to diagnosticate between carcinoma and chancre, or other affections of the lips, but merely to call attention to this inferior maxillary bone which is much enlarged and is surrounded by a mass of carcinomatous growth.

DISCUSSION.

Dr. DeForest Willard stated that about a year and a half before he had removed half of a lower jaw in a case of primary epithelioma, involving the larger portion of one side of the jaw and extending so high that it was necessary to remove it at the articulation. There was infiltration at the same time of the connective tissue and of the glands beneath the jaw, all of which were removed. The patient did very well, but in the course of a few days developed symptoms of meningitis, which were supposed to be alcoholic in character. No account of the man's previous history could be secured, and he was treated for alcoholism. The case progressed with all the symptoms of *mania a potu*, and died on the eighth day. Afterward it was learned that the man never drank at all. The meningitis, therefore, could not have been due to alcoholism, but was probably septic. This case shows what may happen after what seems to be a simple operation.

Dr. Hearn stated that if the saliva contains typical micrococci of croupous pneumonia, and these are the same as those of meningitis, we may have infection after operations about the mouth with saliva, followed by pneumonia or by meningitis.

The President said that there might also be direct infection causing meningitis by absorption from the wound after operations upon the mouth.

Dr. Hearn added that he closed the mucous membrane accurately, so that no discharges from the wound could get into the mouth or the saliva enter the wound. The case recovered.

A CASE OF STAMMERING CURED BY AN OPERATION.

G. HUDSON MAKUEN, M.D.

[Exhibited January 22, 1896.]

More than fifty years ago operations were performed with varying success for the cure of stammering. Among the principal ones were excision of the tonsils, and of portions of the tongue, amputation of the uvula and division of some of the fibers of the hyo-glossus and genio-hyo-glossus muscles. These operations met with opposition on every hand, and justly so, because many of them were done empirically rather than for the correction of definite anatomic defects. To amputate the uvula or excise the tongue in every case of stammering would be, to say the least, very bad surgery.

The child that I shall show this evening was referred to me for examination less than two months ago. The mother said he had a "catch" in his speech. He was nervous, and timid, and poorly nourished, having always suffered from indigestion. To what extent his stammering was the cause of these symptoms, it is difficult to say, but I believe it was one of the chief factors. Any peculiarity of speech makes an immense impression on a child. His parents always make him feel that it is a great calamity, and his playmates poke fun at him; and few children can endure this sort of thing without experiencing great mental depression, with all its attendant physical derangements. The problem presenting itself then in every case of speech-defect is: What is the cause and what is the effect? In other words, does the stammering cause the indigestion, or does the indigestion cause the stammering? We can make no hard-and-fast rule in this matter, but every case must be a study in itself. Take, for example, the one that I presented a year ago. A young man, nineteen years of age, who had never been able to use intelligible speech, and whose general manner was almost idiotic, and health very precarious. It is now a little more than two years since I divided the anterior fibers of the genio-hyo-glossus muscle and thus loosened up his tongue and made speech possible. I then taught him to speak, and in less than a year after the operation he articulated in your presence, "Brutus' Speech Against Cæsar," with almost perfect accuracy. And now, a little more than a year later, he is in George School preparing for college, and I am told by the Principal that he is one of the brightest boys in the school, and his general health is excellent. The first cause in his case was an organic one, viz.: a short genio-hyo-glossus muscle, and this organic and congenital defect was just enough to prevent the development of the speech-faculty; and the non-development of this faculty became in its turn the cause of his stupidity and lack of physical tone.

Stammering, or any other defect of speech, should be looked upon as a disease, or the symptom of a disease, and treated accordingly by the skilled physician, and not, as is so often the case, relegated to some charlatan who claims to have made a wonderful discovery that he retails for so much hard cash, at the same time binding the unfortunate patient to profound secrecy. Any trick

for the cure of stammering, applied to all cases, must be a failure, because no two cases are alike, and no single expedient, or combination of expedients, is applicable to all. The only rational method, of course, is to study each case separately, and if possible make an accurate diagnosis, as we do in any other disease.

Upon examination of the case before you, I found, in addition to what I have already mentioned, a defective tongue-action, an elongated uvula, and what appeared to be a slight adenoid thickening, although my view of the vault of the pharynx with the rhinoscopic mirror was very limited. I observed also that the child had defective vision. I asked him his name and he could not tell me to save his life, though he made frantic efforts to do so. The difficulty seemed to be at the base of the tongue, and even when no attempt was made at speech there were peculiar twitchings of the lingual and facial muscles.

After a careful study of the case at frequent intervals for two weeks, I made a diagnosis of chorea of the facial, lingual, pharyngeal, and laryngeal muscles, due chiefly to adenoid hypertrophies and in part to some deviation from the normal in the genio-hyoglossus muscle and to defective vision. I explained the matter to the parents, and, after a consultation with the family physician, and with his assistance, I put the child under ether and divided the frenum of the tongue well back, and then with my own modification of the Gottstein curet I removed a mass of tonsillar tissue from the vault of the pharynx fully as large as a black walnut. I was surprised at the size of this tonsil, for, aside from the chorea that I have mentioned, he had few of the symptoms of adenoid vegetations.

The child made a rapid recovery from the operation and came to my office on the second day thereafter. Frequent lingual traction was made to keep the cut edges of the frenum from uniting, and the tongue has now a full half-inch increase of protrusion, and the protrusion is almost straight, whereas before the operation it was a half-inch to the right of the median line.

Had the child not improved so much in speech I should have considered the advisability of dividing a few fibers of the genio-hyo-glossus muscle on the right side in order to correct this irregularity, but the improvement has been so rapid I have thought further operative interference unnecessary. He has not had a single "catch" in his voice since the operation. The after-treatment, besides the application of glasses to improve his vision, has been along the line of elocutionary drill directed toward the correction of several little faults of speech that were more or less closely related to the main trouble. For instance, he has had a tendency to talk more rapidly than is compatible with good speech, and as a result of this there has been a frequent repetition of certain syllables. This constitutes a kind of stutter that is very different from the more serious spasmodic muscular contraction that characterized his former attempts at speech, and which is not yet entirely eradicated, although at times it is scarcely noticeable.

Before introducing the boy I shall briefly review the case. His speech had never been good, and for some time past it had been growing progressively worse until it threatened to wreck his future usefulness. Not only were the muscles controlling his vocal and oral expression affected, but those controlling his facial expression as well, as was shown by various kinds of contortions and grimaces. I clipped the frenum of his tongue and removed a large pharyngeal tonsil, and then gave him vocal exercises, the character of which I shall presently show you.

Ten days after the operation Dr. de Schweinitz examined his eyes and sent me the following report:

"Refractive error: hypermetropia, with congenital amblyopia of the right eye, vision amounting, under the best correction, to not more than two-thirds of normal, while in the left eye the vision was entirely normal. Both optic discs were slightly congested, with undue prominence of the central lymph-sheaths and many lymph-reflexes throughout the retina. There was insufficiency of one degree of the external

recti. The ocular symptoms were much wrinkling of the occipito-frontalis and occasional headache. The refractive error was corrected, and it was advised that the glasses should be worn constantly, and that the functions of the right eye should be developed by separate exercises."

This instruction has been carefully followed, and a week ago I removed the tip of the uvula, which was too long and which acted as an irritant to the throat, and as a result the voice is somewhat clearer. The boy has evinced rather more than the average intelligence and will-power, and his mother has assisted him greatly in carrying out my directions.

As I have said, he could not tell me his name, and I shall now show you what he can do after less than two months' treatment.

DISCUSSION.

Dr. John Graham stated that the little boy whom Dr. Makuen exhibited had been under his professional care since birth. His speech had always been defective, but it was hoped, that under the careful training of his father and mother, the stammering would be overcome. He was instructed to try to speak slowly and distinctly; his general health was looked after, but, in spite of all care, he not only did not improve, but, during the past year, he had grown decidedly worse. Lately, he had hardly been able to speak at all, especially before strangers. The consequence has been that, although a bright boy, it had been almost impossible for him to pursue his studies at school, or for his teachers to know what was going on in the child's mind. He was not deaf, but he certainly was almost dumb.

Dr. Harrison Allen said that the interesting observation reported by Dr. Makuen illustrates a phase of study of the adenoid growth which has been in a measure overlooked. The case informs us of the impression that can be made by a growth in the pharynx upon the development of speech; indeed, it may be said that if retarded speech-development or imperfect enunciation of consonants be found in a child who is also suffering from adenoid growth the probabilities are that speech will be improved by ablation of the mass. Dr. Allen was prepared to make an assertion of a yet more radical character. He alluded to the effects of the adenoid growth on mutism. In a mute boy, upon whom he had recently operated, the hearing improved to such a degree as to enable the boy to leave an institution for mutes and to attend public school. The growth in this case was so small that it did not obstruct nasal breathing. In a paper read before the last meeting of the Pennsylvania State Medical Society, Dr. Allen had recorded instances of profound impressions upon general nutrition which were changed for the better by removal of small non-obstructive growths. Indeed, it appears that we are standing upon the threshold of a subject of great importance in the clinical study of development and general nutritive processes in childhood. If the masses are sufficiently large to obstruct the naso-pharynx a diagnosis is easy, but to detect the presence of smaller masses is often difficult unless a digital examination is made. There is one sign present in many cases, namely, large lymphatic glands under the jaw. If struma, tonsillitis and congenital syphilis can be excluded, the presence of these enlarged glands may be accepted as a sign of the existence of an adenoid growth, which is exciting mischief irrespective of its size. It is of interest to note that the enlarged cervical glands will disappear in a few days after operation.

Dr. James Thorington asked what had been the effect upon the digestion in this case, since the operation. As the statement was made that the child was not deaf, he also asked if any record of the child's hearing had been made prior to the operation. With such a large adenoid growth, it could be well understood that there might have been some interference with hearing, and that the removal of the growth materially assisted in securing this excellent result. With regard to the muscle-condition, he asked if there was any deviation in the tongue, either in repose or when protruded from the mouth.

Dr. W. S. Stewart related the case of a man with such defective articulation that it interfered with his occupation. When he came to the doctor's office, he had such difficulty in articulation that the servant was frightened, reporting that there was a crazy man at the door. This man was cured afterwards by the faith-cure. For a time he spoke perfectly and without hesitancy, but afterwards relapsed.

Dr. Jurist, having understood Dr. Allen to say that the existence of enlarged glands in the neck is always evidence of the presence of adenoids, asked if he would extend this statement and say whether or not nasal obstruction in general will not cause enlarged glands.

Dr. Allen replied that enlarged glands in the neck are connected with the existence of adenoids, but there is not necessarily any relation as to the size. This sign is sometimes inconclusive, as all signs may be, but we do not form our opinion from one sign, but from a group of signs. It is true that we may have enlarged glands from nasal disease and from enlarged tonsils, or from scarlet fever, but when there is a suspicion of adenoid growths, the sign is useful. A certain amount of enlargement of the tonsils is physiologic in children; but the tonsils are not always enlarged when there are adenoid growths.

Dr. G. G. Davis stated that it is interesting to know to what degree these cases are dependent upon physical causes, and in this case the removal of a growth the size of a walnut probably had something to do with the improved condition of the child; but he was convinced that a great deal is due to the care taken by Dr. Makuen in training the organs of speech. In some children faults of speech are due to anatomic reasons; but a large proportion will, upon examination, be found to be able to make all or nearly all the sounds, and they would be able to speak much better than they do if they utilized all the power that they possess. Dr. Davis asked how much of the improvement in the case was due to the operation and how much to the training.

Dr. Laplace said that he was much entertained by hearing the little boy read his exercises, which he has practised very assiduously. It was interesting to see how fluently he read them, and it was very instructive to observe that he talked as well as he read. The method of cerebral action is different in speaking from that of reading, and stammerers are likely to have more hesitation in expressing their own thoughts.

Dr. S. Solis-Cohen stated that, if he understood Dr. Makuen correctly, there were several contributing factors causing the speech-defect in this case, all of which were corrected. Unless Dr. Makuen's observations are directly to the contrary, more stress is to be laid upon the tongue-tie than upon the adenoid vegetations, for too many cases of adenoids are without such difficulty of speech to readily believe that this pathologic condition is a true cause of stammering or stuttering, although its removal will relieve the patient of very many other annoyances. With Dr. Davis, Dr. Cohen attributed much of the good result to Dr. Makuen's skilful training. His results are exceptional because exceptional skill was brought to bear.

In view of Dr. Allen's remarks concerning the effect of adenoid growths upon the mental condition, reference was made to the results of examinations of feeble-minded children at Elwyn. They showed an unusually high percentage of these growths. Dr. J. Solis-Cohen has operated upon several such cases in private practice, with the result of apparently improving the mental condition. It was thought that these growths, while not causative, are concomitants of arrested or perverted development, and their removal, by affording relief to respiration and increasing hematosis, removes one of the obstacles in the way of the development of the body and mind of the individual.

Dr. Makuen concluded by saying that there were no enlarged glands except in the vault of the pharynx, and the tonsils were rather smaller than in other children of the patient's age. The suggestion that stammering may continue after the original cause has been removed is a good one, and it is here that vocal training plays an important part. The stammering-habit must be broken up. As to the question as to the relative value of the operation and of training, in this case they were about of equal value as curative agents, and in the majority of operative cases the one would be almost useless without the other. The habit must be removed, else the patient will go on stammering, although this boy has not stammered since the operation.

Very little personal attention had been given to the training of this case, but the mother has been very diligent in carrying out instructions given her, and the boy has enjoyed the exercises and taken a great interest in them. When the boy was asked, three weeks previously, to come before the meeting, he was much disturbed. He had no confidence in himself whatever, but that was restored entirely by simply teaching him to talk. His general health had also improved and his mother said that his appetite and digestion had never been so good.

As regards the relative value of the two operations, little credit need be given to the clipping of the frenum of the tongue because this was not very short and seemed to interfere but little with the tongue's action. We can scarcely over-estimate the importance of good speech as a factor in the mental and physical development of children. It stimulates all the faculties and improves the general health to a marked degree.

Dr. Makuen added that he had sometimes thought that adenoid growths and speech-defects may be causative factors rather than effects in strumous conditions.

THE IMPORTANCE OF PRECISION IN THE TECHNIC OF HYDROTHERAPY.

[ABSTRACT.]

SIMON BARUCH, M.D.

[Read February 12, 1896.]

Dr. Baruch pointed out that when he made his first plea seven years ago for the employment of the Brand bath in the treatment of typhoid fever not a single voice was raised in his support. A few days later, however, he received an invitation to instruct the medical staff in one of the divisions of Bellevue Hospital in the technic. Encouraged by the gratifying results that have been obtained since that time, Dr. Baruch has been led to employ hydrotherapy not only in acute diseases, such as typhoid fever, pneumonia and the exanthemata, but also in chronic diseases, such as tuberculosis, rheumatism and gout, in which the nutritive tissue-changes or hematosis is defective. The virtue of water as a therapeutic agent resides in its flexibility, inasmuch as by a judicious adaption of temperature, duration and pressure, it may be applied in conditions of the greatest feebleness and variety.

In the average medical mind the following objections to hydrotherapy at once arise: (1) The difficulty of applying water systematically and with precision, from the lack of facilities and the natural repugnance to water on the part of most persons. (2) The adoption of water as a remedy by empirics and ignorant laymen who have brought it into disrepute. (3) The utter disregard of exactness of technic. The last is probably the chief cause of the indifference of the profession, as failure will surely result from inattention to details. It is a too common error to confound all kinds of cold bathing and sponging with the Brand treatment. It has been definitely ascertained that high temperature is not the chief danger to be feared in an attack of typhoid fever. The object of the Brand bath is the support of the nervous system, with a secondary consideration for the temperature. A longer bath at a higher temperature would also reduce the bodily temperature, but would be lacking in the nervous stimulation. Friction of the body and cold affusion of the head augment the influence of the cold bath, which is appreciable in the improved action of the heart, lungs, and secretory organs. To obtain the ideal result a precise technic must be followed. This consists in beginning the friction-baths at 65° F. before the fifth day of the disease and continuing them without fail every three hours, night and day, when the patient is awake, as long as his temperature reaches 102.5° F. The bathing cannot begin too early, and no harm can result should the case prove not to be one of typhoid fever.

The Brand bath may at times serve as a diagnostic guide. Whenever the morning temperature is above 102° F. on the third or fourth day of a febrile disorder and it rises 1° F. or more in the evening in the absence of a local cause, and a bath at 90° F. with friction for fifteen minutes, repeated in four

hours at 80° F. and then at 75° F., causes the rectal temperature to fall 2° F. or more, typhoid fever can almost positively be excluded.

In the treatment of pneumonia, and especially the broncho-pneumonia of very young children, excellent results have been secured by the use for ten minutes with friction of the tub-bath at a temperature of 95° F., gradually reduced to 80° F. during the first bath. Succeeding baths are repeated at intervals of four hours if the temperature reach 103° F. or more. In the treatment of pneumonia in adults the results from this method, and also from the strict Brand bath, were unfavorable. The following procedure, however, has proved entirely satisfactory. The patient receives first from ten to twenty grains of calomel, and is carefully sponged with soap and water and dried. Two compresses of three thicknesses of old crash or linen towelling or sheeting are wrung out of water at a temperature of 60° F., and neatly applied to the chest, being covered firmly with a flannel bandage. The application is renewed every hour as long as the temperature ranges between 100° and 102°, and every half-hour if the temperature be higher. Free libations of water are permitted and strychnin is employed hypodermically if the action of the heart is feeble.

The judicious employment of water is also capable of excellent results in the treatment of pulmonary tuberculosis. The object here is the improvement of the nutrition. This is effected by first dilating the superficial cutaneous vessels and thoroughly warming the patient by a dry pack, *i. e.*, enveloping him in long-haired blankets, or by a hot-air bath, short of inducing perspiration. Then he is subjected to a circular or needle bath under a pressure of twenty-five pounds, at a temperature of 95° F., gradually reduced in the course of one minute to 85° F. and followed by a fan douche under a pressure of twenty pounds, at a temperature of 90° F., reduced daily 1° F. If precision in the technic be not observed and the patient is exposed to lower temperatures or subjected to higher pressures at the beginning the results will be serious and discouraging.

In cases of neurasthenia, atonic and nervous dyspepsia and simple chlorosis the results obtained with hydrotherapy applied with judgment and skill in improving the nutrition and hematosis are striking. A case of chlorosis may be instructed to stand in a tub containing twelve inches of water at a temperature of 100° F., while an attendant squeezes from a crumpled towel or two bath-gloves upon successive parts water at a temperature of 90° F. or less, according to the reactive capacity of the patient, meanwhile rubbing gently the surface of the body. The patient is then gently dried with a soft linen towel and subsequently rubbed with a crash towel. The temperature of the bath should be reduced 1° or 2° F. daily until 50° is reached; then larger quantities of water should be poured over the body by squeezing a large sponge quickly over different parts. Later pitcherfuls or basinfuls may be poured over the body with some force.

DISCUSSION.

Dr. Jas. Tyson stated that his own experience with hydrotherapy was almost entirely limited to the cold tub-bath treatment of typhoid fever. He was a firm believer in this treatment, and it is the only treatment which should be practised to-day when possible. The day is approaching rapidly, the hour almost is here, when the objections of physicians will be at an end. The chief difficulty at the present day lies in the objection of the people, the great and unwholesome dread of water, and the difficulties in carrying out the treatment. The latter are still very, very serious, more particularly, of course, in connection with private practice. There is no longer any trouble in hospital practice. The difficulties in the former are diminishing by the use of the portable bath, and of all these baths that suggested by Dr. Burr, of Chicago, seems to answer better than others. Dr. Tyson has one of these tubs in his possession and twice in the past winter he has loaned it to physicians, and each time its use has been followed by the recovery of the patient, while those

having used it have expressed the utmost satisfaction, and a firm determination to employ the treatment whenever it was possible. Dr. Tyson admitted that he had himself unknowingly modified the technic as given by Brand, though he did not think this has interfered with the success of the treatment. For a long while Dr. Tyson was under the impression that the Brand treatment consisted in giving a bath at 68° F. whenever the temperature exceeded 102°. It will be remembered that the Brand direction is to give a bath only *once in three hours* whenever the temperature exceeds 102°. Now, when we consider that patients have at the beginning a great horror of the treatment—and certainly it is very uncomfortable at first—it is evident that it is desirable to make the baths just as few as possible and as consistent with the highest success of the treatment. In Dr. Tyson's experience the mortality has been seven per cent. —that is higher than Brand reports, about what Osler reports, and a little higher than Dr. Wilson reports, but the result is largely due to the late date at which most of the patients come under treatment. One rarely has the opportunity of treating them earlier than the fifth day. Dr. Tyson used to wait for the diagnosis, a mistake that he has learned to correct and he now bathes as soon as there is a suspicion of fever, as the bath cannot do harm.

In regard to the mode of action, the explanation of Dr. Baruch and others may be accepted, although it is difficult to give up altogether the idea that there is something also in the reduction of temperature which helps the good effect, though it is true, as Dr. Baruch says, that antipyretics of the cold-tar series also reduce the temperature without the same happy effect. One of the striking features in the treatment of these cases is the almost total absence of what goes to make up what we call the typhoid state, such as stupor, delirium, the dry tongue, etc.

Dr. Tyson's experience with hydrotherapy outside of typhoid fever was most *nil*. The treatment of pneumonia by ice he had tried in a few instances. The difficulty had been in keeping the cold dressing applied. The method suggested by Dr. Baruch of fastening the dressings to the chest is worth trying.

Dr. H. A. Hare pointed out that in the time of Hippocrates fever was considered a conservative process, although later it was viewed with concern. It has been experimentally shown that animals inoculated with infective matters have a better chance of recovery when fever develops than under the reverse conditions. It is not perfectly clear in what way fever exerts its beneficent influence. This may possibly have some connection with leukocytosis, as it has been shown clinically that cases of pneumonia attended with an increase in the number of leukocytes have a more favorable prognosis than those in which such increase fails to take place. It is probable that the good effects resulting from the application of cold in the treatment of pyrexia is not due to the abstraction of heat. With the contraction of the superficial vessels there is an afflux of blood to the interior of the body, with consequent elevation of temperature and activity of the internal viscera. In this way it is probable that the well-known capacity of the liver to destroy toxic substances is increased; leukocytosis also results and increased exudation-processes occur, which in turn perhaps destroy the micro-organisms or their toxins.

Dr. Fenton B. Turck, of Chicago, also emphasized the importance of precision in method in every detail in the application of water in the treatment of disease. In this connection must be considered the temperature of the bath and its duration, the object to be accomplished, the individuality of the patient and his tolerance of the treatment.

Dr. J. M. Anders stated that the profession is convinced of the efficacy and value of the Brand method of treatment. A large number of writers, however, adhere to the opinion that it can be carried out only in hospitals. This is the reason why substitutes for the cold bath are being used, such as sponging, the cold pack, etc. Dr. Tyson and Dr. Baruch said correctly that there are serious obstacles to this treatment in private families. Telling persons in families in which typhoid and other infectious diseases occur to add alcohol or vinegar to the water helps to overcome the prejudice which exists still against the use of cold water. The portable bath-tub has also done much to remove this prejudice. According to Dr. Anders' experience, however, although it is possible to give a gradually cooled bath by means of a portable tub while the patient is lying abed, one cannot very well give a genuine cold bath. To do this the tub must first be filled and the patient immersed immediately up to the neck. The rigid Brand method serves to stimulate more potently the nerve-centers than the gradually cooled bath. Dr. Anders uses the former in severe types of infection; the latter in mild types.

The duration of cold baths at a temperature of 70° or 65° F. should not be prolonged on the average beyond ten minutes; a gradually cooled bath not beyond 15 or 20 minutes. Dr. Anders recalled a case in which the heart was very weak, and which ended fatally soon after a protracted cold bath. An *excessively* feeble heart is a contra-indication to the cold bath, as well as peritonitis and hemorrhage from the bowel. In pneumonia Dr. Anders has had some experience with the cold-bath mode of treatment. In two cases disastrous results followed, and since Dr. Anders has limited himself to the use of cold, locally.

Dr. Wharton Sinkler said that there was no question of the efficacy of hydrothera-

py in neurasthenia. The method of applying hydrotherapy must be adapted to the form of neurasthenia to be treated, and, also, individual peculiarities must be considered. When there is marked insomnia the use of the hot bath at bed-time, or, in some cases, the wet pack, will afford relief. In forms of neurasthenia, attended with spinal pain, the alternate cold and hot douche is beneficial. Care must be taken, however, in selecting cases or mischief may be done. In feeble and anemic patients it is unwise to apply much cold, and in cases in which there is much nervous perturbation the douche is often harmful.

In regard to the subject of bathing in fever, Dr. Sinkler remarked that the application of hot water was efficacious in reducing the temperature. In one patient he had seen the temperature reduced by hot sponging from 104° to 102° in a half-hour.

Dr. Thos. J. Mays said that he was glad to hear the protest against shock. Nothing interferes so much with the successful application of cold in any disease as the fear of shock. He had applied cold for a number of years to a great extent. He had never found a single instance of shock in the application of cold in pneumonia, to which disease he limited his remarks. While Dr. Baruch said that the local application of cold in pneumonia is preferable to its general application, Dr. Mays, from his own experience, thought this was correct. He maintained that pneumonia is one of the diseases in which the general application is not the best. Cold sponging is not sufficiently heroic, and the bodily annoyance necessary to give a cold immersion disturbs the patient's rest too much, exhausts his strength, and reduces his vitality.

Another question that has been referred to by Dr. Baruch, is whether the curative effects of cold come through the nervous system alone. Dr. Mays expressed himself a firm believer in the important role that the nervous system plays in the production of pneumonia, and had no doubt that some of its good effects come through this channel; yet he who has watched the local action of cold on the inflamed pulmonary area beneath, cannot help knowing that cold has a profoundly favorable influence on the inflammatory process. This has often been observed. Cold not only suppresses the inflammation, but prevents it from spreading any further in many cases.

It is not a very difficult matter to maintain the ice-bags in position. The mode of application, while the most available, is yet capable of improvement. The ice-bags are partially filled with ice and as many put on as will cover the seat of inflammation. The bags are put next to the skin, wrapped in towels, and a roller or towel strapped around the patient. If the inflammatory process is very intense, or the fever high, the ice-bags are applied without being wrapped. They are never all taken off at once for the purpose of filling them. They are filled at different periods. If the temperature falls very suddenly the ice-bags may be withdrawn at once. Great caution must be exercised, however, on this point. Frequently the temperature is markedly depressed, but if the depression does not anticipate a crisis, and the ice-bags are taken off, the temperature flies up again and is harder to bring down than it was in the first place. Hence it is best to remove the ice gradually. When the crisis arrives or is here in its fullness, the ice is taken off and warmth is applied to the whole body, and alcohol in large quantities internally.

In closing the discussion, Dr. Baruch repeated that the primary object of the cold bath in the treatment of the febrile process is not the reduction of temperature. It was from this point of view that Liebermeister, though more successful than those pursuing the expectant plan, failed for a long time to secure the ideal results of the Brand method. Prolonged baths at low temperatures, with quinin during the night, to keep the temperature down, proved inadequate. Feebleness of heart must be considered a distinct indication rather than a contra-indication for cold bathing in typhoid fever. In the most perfect form of heart-failure, as in syncope, the first expedient is the use of cold water. In the treatment of scarlet fever with feeble heart-action, as indicated by the cyanotic marbled skin, it is often useful to immerse the child quickly in water at 75° and then to gently dry it. The reaction is shown by the glow that follows. The inadequacy of ice to influence the local process in cases of pneumonia and peritonitis is shown by the observation that after application of ice to the abdominal walls until nearly frozen, no appreciable influence upon the inflammatory process was noticed on autopsy.

ANNUAL ADDRESS OF THE PRESIDENT.

JAMES C. WILSON, M.D.

[Read February 26, 1896.]

It is one of the agreeable duties of the President of the Philadelphia County Medical Society to submit, at the close of the year, in the form of an address, a brief review of the work and condition of the Society. This formality, which the custom of my predecessors in this Chair has made a simple and concise statement of facts, is not without its usefulness. It brings to the attention of members matters of importance that otherwise many of them would overlook; it enables us to estimate the general value and importance of our work in the Society, our usefulness to the local profession, our weight with the State and National societies to which we send representative delegates, to compare the current events of the Society with its past, and finally to lay plans for greater usefulness in the future. I deem myself fortunate in being able to say to you that the Philadelphia County Medical Society has fully sustained, during the year that has just closed, its record of usefulness. The number of its scientific meetings was eighteen, one having been omitted in May, as coming into conflict with the session of the American Medical Association, and one stated meeting having been omitted in December, as falling upon Christmas. The four regular quarterly business meetings were held. The attendance at the meetings has been large. On more than one occasion this hall has been crowded to its full capacity. The average attendance at the scientific meetings has been fifty-three; the maximum, 121. During the course of the year forty-three new members were elected, of whom, at the close of the year, thirty-two had already qualified. The number of papers read during the year was fifty-five. A critical review of these communications shows that the majority of them are of the highest scientific and practical interest. No meeting has been without important and instructive papers and discussions of great suggestiveness and value. There has been the freest interchange of views among men whose training and experience justify explicit expression of opinion, but in no instance has there been the slightest violation of the courtesies of debate. A number of the papers have been devoted to the presentation of recent facts not generally known to the profession. Among these was the paper of Dr. Roussel upon "The Laborde Lingual-Traction Method in Cases of Threatened Asphyxia," that of Professor Parvin upon "Schleich's Method of Local Anesthesia," and that of Dr. Fullerton upon "Posture in Difficult Labors." These papers and the discussions which they elicited proved interesting and instructive to the members of the Society.

The public spirit of the Society has been manifested in decided action upon questions relating to preventive medicine and the welfare of the profession. The Philadelphia County Medical Society has, during the past year, appointed committees and taken action on a law upon the subject of ophthalmia neonatorum, upon the matter of private hospitals for contagious diseases, upon the status of medical men in the navy;

while a powerful committee has been appointed, consisting of one member from each ward in the city, to take action in the matter of irregular practitioners. The Society has made its power and influence felt in the meetings of the State and National societies by the character of its delegates and their intelligent action upon questions relating to the welfare of the profession and the extension of its usefulness. Particularly was this the case in the matter of Dr. S. S. Cohen's committee upon advertisements in the *Journal of the American Medical Association*.

Thus we have much upon which to congratulate ourselves. We are, however, far from being content. Restlessness and activity are necessary to progress. This is not a conservative, but a radical society. It does not rest upon the laurels of its past, but strives for larger and broader influence in the activities of the future. To realize the best aims of the Society, its individual members must take personal interest in it. To attend its meetings, to bring to it in the form of papers and reports of cases the best and most recent observations, to take part in the discussions, are not enough. The members should speak of the Society to their friends, should make clear its purposes, should show that it is in the highest sense the local exchange of all that is best and most useful for the practitioner. We need a larger membership, a finer sense of professional brotherhood. Every regular practitioner within the limits of the county should be enrolled upon our list of members. So nearly should this be the case that to fail of election to the County Medical Society should be regarded as a disgrace, and I for one believe it would be well if every member of this Society wore, as do the members of military societies, upon the lappet of his coat, a button, as the insignium of membership in the most liberal and most useful of the medical societies of Philadelphia. In such an organization as this there are the means of elevating the tone of the profession, of increasing the knowledge of its individual members, of influencing public opinion in all matters relating to preventative medicine and sanitation.

I desire to call the attention of the Society to the fact that the date of the last edition of the Constitution and By-Laws is 1887. A new edition, embodying the changes to date, is needed. The matter is now in the hands of a committee and it is hoped that a report will be ere long forthcoming.

The thanks of the Society are due to the Directors of the past year for having brought to its attention the exact condition of its finances. Owing to methods of book-keeping which involve no criticism of former officers, it has come to pass that we find ourselves in debt. The Society has acted with honesty and decision in abandoning the publication of its *Transactions* for the current year in order to meet obligations incurred in the past.

My distinguished predecessor in th s Chair, in his final address, raised a question concerning the advantage of changing the hour of meeting from 8.00 to 8.30. The reasons he adduced in advocating this change appeared to me at that time cogent. My experience during the year, however, has led me to believe that such a change should not be made without further careful consideration. Most of the meetings have been fully attended and a majority of the members have, as a rule, entered the hall within a few minutes of the hour named for the opening of the Society. Notwithstanding this, many of our meetings have been late in adjournment, often far past ten o'clock. In this connection I venture to suggest still further conciseness in the presentation of papers and in the discussions.

It is my painful duty to recall to the memory of the members the deaths during the year 1895 of their colleagues, Drs. S. K. Ashton, James Collins, Lewis D. Harlow, Albert G. Heyl, J. W. Hughes, George A. Rex, J. W. Brockbank, A. M. Hamilton, J. D. Schoales and G. J. Ziegler.

A CASE OF INDIGENOUS PARASITIC CHYLURIA, WITH FILARIA NOCTURNA IN THE BLOOD.

FREDERICK P. HENRY, M.D.

[Reported in abstract, February 26, 1896.]

Fanny B., a married woman, twenty-nine years old, was born in Columbia, S. C., and spent the first twenty-seven years of her life in that town. Her twenty-eighth year was passed in Palatka, Fla., and her twenty-ninth in Philadelphia, where she arrived in March, 1895. Her father died from sunstroke, and her mother, for several years before her death, suffered from "shingles"—herpes circinatus. She had the usual diseases of childhood, except scarlatina. At the age of twelve she fell from a fig-tree, and soon afterward developed a large abscess in the left lumbar region, the site of which is plainly indicated by a cicatrix about three inches long, a little above and parallel with the posterior portion of the crest of the ilium. This abscess continued open for several months, but finally healed. A year after its closure another abscess appeared in the left iliac region, and also pursued a chronic course. Its site is indicated by a linear cicatrix, about two inches long, a little above and parallel with Poupart's ligament.

The patient suffered from what she vaguely described as an attack of malarial fever in March, 1895, but never manifested any signs of malarial infection while in the South. She has had two miscarriages—one at four months and a half; the other at two months. Both were ascribed to persistent vomiting. Three weeks before her admission to the Woman's Hospital of Philadelphia, on February 16, 1896, she gave birth to a child at term. The labor was natural in all respects. Previous to the birth of her child she suffered from pain in the region of the kidneys. On the second day of her lying-in this pain become intense and continued for a week, when it abated somewhat. On admission, it was still complained of. On the third day of her lying-in she passed milky urine, and had difficulty in micturition on account of the occlusion of the urethra with what she regarded as stringy masses of mucus. These were, in reality, coagula of lymph and blood. The urine, after standing for several hours in a narrow cylindrical vessel, separates into two portions, of which the lower is distinctly hemorrhagic; while the upper has the appearance of milk or cream. Floating on the upper chylous layer are numerous coagula of a delicate, pinkish hue, and almost translucent, while at the bottom are a few small blood-clots. A little of the urine was shaken up in a test-tube with ether and set aside until the urine and ether had separated. The latter being then evaporated on a watch-glass, a distinct deposit of fat was obtained. The chylous urine contained a trace of albumin, but no sugar, and was free from casts.

I found this woman awaiting me at my clinic at the Woman's Hospital on Tuesday, February 18, 1896, and lectured upon her case as one of chyluria, probably parasitic. On the evening of February 19th I visited the hospital for the purpose of examining my patient's blood for the filaria sanguinis, taking with me a small microscope and making

the search with a half-inch objective. I withdrew the blood from the finger at about ten P. M., and examined several slides without finding the parasite. I left the slides at the hospital, and after my departure one of the resident physicians, Dr. Ida E. Blackburn, examined them with a stronger lens, and fortunately succeeded in detecting the filaria. Since then filariæ have been found in almost every slide examined. They are not numerous, the maximum number observed on a single slide being five. The urine was repeatedly examined, the centrifugal machine being used to separate the parasites, but only on one occasion were they found in that fluid. With the specimen in which they were detected the centrifugal machine was not employed. The filariæ have not been found in the milk of the mother or in the blood of the infant, and they are very few in number or absent from the blood of the mother during the day. The variety present in this case is, therefore, the filaria nocturna, the embryo of an adult which is alive in one of the lymphatic channels.

The patient was put to bed, and frequent examinations were made of her blood and urine. She was at first placed upon quinin and ergotin without any apparent effect, the urine continuing chylous and bloody, although in an intermittent manner. On February 25th, I examined the blood, and found filariæ. I then directed leeches to be applied to the lumbar region, ostensibly to relieve pain, but in reality to test the question whether the leech might play the rôle of an intermediate host to the filariæ. Three of the leeches were sent to me the next morning. I opened one of the leeches at ten A. M., and examined its blood. Filariæ were abundantly present, one slide containing six in active movement. The next morning (February 26th) they were still active. The same afternoon I found three dead filariæ on one of the slides, and but one still living and languidly moving. *At* the same hour the filariæ removed directly from the body were all living. On February 29th I opened another leech, and found a number of dead filariæ in its blood—none living. It appears evident, therefore, that although the filariæ may live many hours in the body of the leech, that animal does not play the part of an intermediary host to them.

On February 28th, the patient was placed upon thymol (gr. ii every three hours), and coincidently with its administration, the urine became normal in every respect, and so continued for seven days, when it again become chylous and bloody. The filariæ during this interval were abundantly present in the blood.

On March 12, I ordered methylene-blue in two-grain capsules every three hours, being induced to do so by the remarkable statements of Dr. Austin Flint concerning the efficacy of this substance in a case of parasitic chyluria.[1]

On March 13, I found Mrs. B. out of bed and dressed. Her appearance was good, her lips and cheeks being well-colored; the urine was deep blue; the eye-ground was examined by Dr. Gertrude A. Walker, opthalmologist to the hospital, who confirmed my observation as to the absence of any morbid appearance in the retina. The patient was anxious to go home, but was persuaded to stay another week.

On Saturday evening, March 14, I obtained some blood from the finger as usual, and examined it the next morning. I had scarcely placed the first slide under the microscope when I detected two filariæ (in the same field) moving with the greatest activity. I was unable to perceive that the filariæ were stained in the slightest degree by the methylene-blue which the patient at the time the blood was withdrawn, had been taking continuously for seventy-two hours. Her urine and feces were stained a deep blue, but *the milk was uncolored.* Thus far I have found no corroboration of Flint's statement that methylene-blue stains the filariæ in the circulating blood, much less that it exerts any deleterious influence upon them. My experience, although differing from that of Flint, in this matter, is precisely in accord with that of Laveran[2] who

1 *New York Medical Journal*, June 15, 1895.

2 *Bulletins et Memoires de la Soc. Med. des Hopitaux de Paris*, 3 serie, tome x, p. 738.

found that the filaria perished a few seconds after it was brought into contact with a drop of a solution of quinin, of the strength of 1-1000, while methylene-blue (strength of solution not stated) does not hasten their death, and does not stain them until they are dead.

On March 17, Mrs. B. came downstairs to my clinic at the Woman's Hospital; a specimen of her urine, deeply stained with methylene-blue, was exhibited. Blood was withdrawn from her finger at one o'clock, and seven slides prepared. These were repeatedly examined by myself and an assistant, the result being that two filariæ were found in the seven slides. This was the third time the blood had been examined by day, namely, once before at one o'clock, when no filariæ were found, and once at eight A.M., when only one was discovered. It is evident that the parasites are much less numerous in the superficial capillaries by day than by night. This was the sixth day since the treatment with methylene-blue was instituted, and the results, thus far, were by no means encouraging. The drug appeared to be absolutely inert, so far as concerns the destruction of the filariæ. Different opinions as to whether or not the filariæ were stained, were expressed by those who saw the specimens. The majority thought they were not, but being on the lookout for such staining, I *fancied* that they had a faint bluish tinge.

On March 19, at one o'clock, the patient having been taking methylene-blue (two grains every three hours) for one week, I counted the blood-corpuscles. The number of red corpuscles per cubic millimeter was 4,100,000; the white were not increased in number and were *unstained*. The hemoglobin-estimation was sixty-five per cent.

Five slides of rapidly dried blood were prepared, and no filariæ found. It is a singular fact that on the previous day, while the patient was taking the methylene-blue as usual, the urine suddenly became quite clear and macroscopically normal.

I gave two slides of blood to Dr. Alfred Stengel, of the Pepper Laboratory of Clinical Medicine (University of Pennsylvania), in order to obtain his opinion as to whether or not the leukocytes were stained with the methylene-blue, which the patient had been taking continuously in full doses for more than one week at the time the blood was withdrawn. Dr. Stengel reported that he could find no evidence of blue discoloration of the corpuscles.

On March 20, at nine P.M., I again prepared a number of slides. The filariæ were abundantly present. I found them in eighteen out of twenty-one preparations, and, as I did not use a mechanical stage, it is possible that I may have overlooked them in the three slides in which the search was ineffectual. The serum of the blood was decidedly blue, and the filariæ of an exceedingly delicate bluish tinge. The methylene-blue had been taken by the patient continuously in full doses for nine days and had proved absolutely inert, so far as any influence upon the vitality of the embryos is concerned.

On March 21, the patient returned to her home. I omitted to state that shortly after the patient's admission I had her vaccinated, on the theory that an intercurrent infection might destroy the parasite. The vaccination was perfectly successful, but quite as useless from a therapeutic standpoint as the methylene-blue.

The foregoing case is of special interest, both because it is the first of the kind observed in Philadelphia, and for the reason that it adds another to the list of those indigenous to the United States. It is impossible to say how long the filarial embryos have been circulating in the blood of this patient, but it is in the highest degree probable that the infection occurred either in South Carolina or Florida, and it is not impossible that the lumbar and inguinal abscesses, from which she suffered at the age of twelve, were due to the filariæ. Similar abscesses form part of the clinical history of filariasis. The exciting cause of the chyluria was probably the rupture of a dilated lymphatic during the expulsive pains of labor. The supposition that infection occurred at the age of twelve or earlier necessarily implies the circulation of the embryos in the blood for many years without giving

rise to symptoms. In connection with this question of the innocuous presence of the filariæ in the blood of men the following case is of interest:

In the autumn of 1893, a well-known physician of Philadelphia consulted me about his son-in-law, who had resided for some years in Columbia, S. C., and latterly near Tampa, Fla. Filariæ were said to have been found in his blood by de Saussure of Charleston. Up to the time I speak of the symptoms had been those of intestinal indigestion, consisting chiefly of great abdominal distress, meteorism, irregular action of the bowels, great nervous excitement at times, *especially toward evening*. I examined the patient's blood in vain for the filariæ, and Professor Guitéras, who examined it twice, was equally unsuccessful. The time of my examination was about ten P.M., and I have since thought that my failure to detect the parasite may have been due to the fact that the patient walked to my office. The gentleman in question returned to his home in Florida, and a few months later passed chylous urine for the first time. The chyluria continued for one or two months, and has not returned. Of late the patient has been in a fair state of health, and free from the intestinal symptoms mentioned.

Post-mortem examination of those who have perished from parasitic chyluria has revealed enormous distention of the lymphatic vessels of the urinary tract, and sometimes also of the thoracic duct. A few months ago I exhibited before the Philadelphia County Medical Society a specimen of chyluria from a Cuban, whose blood I vainly searched for the filaria. I had but one opportunity of examining the blood of this man, and for that I was indebted to Dr. Charles W. Coburn, who was in attendance upon the case. Shortly after my examination the man died and an autopsy was held under very unpropitious circumstances. There was, however, no difficulty in ascertaining that the lymphatic vessels, especially those of both renal regions, were enormously dilated and convoluted, many of them being of the caliber of an ordinary lead-pencil. The dilatation was most marked on the right side, and in the pelvis of the corresponding kidney there was a pale lymph-clot similar to the coagula passed with the urine during life. In this case it is greatly to be regretted that a careful dissection with a view to the detection of one or more of the adult filariæ was not possible. The time at our disposal was limited, and the light was derived from a single lamp which was held by turns by Dr. Coburn and myself, the autopsy being skilfully performed by Dr. Bundy of the Women's Medical College of Pennsylvania.

The importation of a case of filariasis into a city of the latitude of Philadelphia naturally raises the question whether the disease may become endemic therein, and there seems to be no good reason why it should not. The brilliant researches of Dr. Patrick Manson have established the fact that the mosquito plays the part of an intermediary host in conveying the *filaria nocturna* from man to man. At night the embryos swarm to the surface, while during the day they retire to the deeper vessels. Acting upon this knowledge, Manson exposed a filarial patient to the bites of mosquitoes, and found the embryos in the bodies of these insects, in which, in the course of from five to seven days, they attain a length of one-fifteenth of an inch. In the blood of man they measure from $\frac{1}{80}$ to $\frac{1}{70}$ of an inch, and are enclosed in a sheath, from which they make their escape in the viscid blood of the mosquito. The mosquitoes with the embryonic filariæ in their interior seek water in which to deposit their eggs. This function accomplished, they perish; the embryonic filariæ are liberated, and, through the medium of the water in which they exist, gain access to the human system. One or more of the ingested parasites attain maturity in the lymphatic system and continue for an indefinite period (in some cases for many years) to produce swarms of embryos. The latter, being but $\frac{1}{3500}$ of an inch in diameter, readily traverse the lymphatic glands, and reach the blood-vessels via the thoracic duct. It is through the plugging of the lymphatic vessels, especially those connected with the urinary tract, that the

lymph and chyle become mingled with the urine. For further details concerning the life-history of this parasite, and the mode in which it occludes the lymph-channels, the reader is referred to the writings of Manson.*

In Philadelphia, mosquitoes are abundantly present during the summer and autumn, and have convenient access to the Schuylkill river, from which our water-supply is derived. The chance of a given individual becoming infected through the medium of a river of the volume of the Schuylkill is doubtless infinitesimal, but no one acquainted with the wonderful vitality of the embryonic filaria can deny its possibility. The surest safeguard against this and other sources of infection is filtration.

The *filaria nocturna* is now known to be indigenous in Europe, as appears from the report of a case recently studied by M. Font, of Spain.† The patient was a man, thirty-five years of age, who had resided all his life at Canet de Mar, with the exception of a short period passed at San Sebastian and Victoria. Canet de Mar is a town of 5000 inhabitants, on the shore of the Mediterranean, in latitude 41° 37′ north, between Barcelona and the French frontier, and is a favorite retreat for veteran sailors, many of whom have visited the West India Islands. Dr. Ballester, in a communication to Font, reports having seen in the same town two cases of hematochyluria during fourteen years, in neither of which was there an examination of the blood. In Font's case the presence of the *filaria nocturna* was repeatedly demonstrated.

Thus far three species of filaria have been certainly detected: (1) *Filaria diurna*, (2) *Filaria nocturna*, (3) *Filaria perstans*. These names are indicative of the habits of the animal, the *filaria diurna* being found in the superficial vessels solely or chiefly during the day; the *filaria nocturna* solely or chiefly during the night; while the *filaria perstans* is constantly present in the capillaries of the integument. The *filaria diurna* and the *filaria perstans* are confined, thus far, to the west coast of Africa and adjoining districts; while the *filaria nocturna* is pandemic in the tropics and endemic in certain sections of the United States. The adults of *filaria nocturna* have been frequently found; that of *filaria perstans* never, so far as I have been able to ascertain. In the opinion of Manson, the *filaria loa* of the eye of the negro of Old Calabar is probably the adult form of the *filaria diurna*. If it is not, he argues, then there must be another blood-worm yet to be discovered, for the embryos of the *loa* must escape from the body of their host through the medium of the circulation. The *filaria perstans* has been practically proved by Manson to be the cause of the fatal "sleeping sickness" of the Congo region.

While engaged in writing this article, my attention was called by Dr. Charles A. Oliver, of Philadelphia, to a remarkable case of *filaria loa*, recently reported by Dr. Argyll Robertson. The patient was a lady who had spent eight years in missionary work at Old Calabar on the west coast of Africa. Without entering into the details of this interesting case, I will merely state that in two successive operations Dr. Robertson extracted two *filariæ* (variety *loa*) from the ocular tissues, the first a male, the second a female. Both of these adult parasites are described by Manson in the course of Robinson's paper. The female was stuffed with embryos, but repeated examinations of the blood failed to detect any embryonic filariæ in that fluid. The latter fact certainly seems to refute Dr. Manson's hypothesis that the *filaria loa* is the adult form of the embryonic *filaria diurna*.

In a letter recently received from Dr. Manson, he says that America possesses the "unenviable distinction of possessing a filaria of the blood, which is possibly peculiar to itself. I found it in negroes from the island of St. Vincent, and I have little doubt but that it could be found in the negroes of the more tropical States of the Union. This filaria I have named *Filaria Demarquayi*, after Demarquay, the discoverer of *filaria nocturna*. It is a very small worm, not

* Especially the articles in Davidson's "Hygiene and Diseases of Warm Climates;" *International Clinics*, April 1895; "Transactions of the International Congress of Hygiene and Demography," Seventh Congress, 1891.

† *Revista de Ciencias Medicas de Barcelona*, 25 February, 10 Marzo, 1894.

half the size of the filaria you are familiar with. It observes no periodicity; it is sharp-tailed, and it possesses a sheath," etc.

This *Filaria Demarquayi** should, therefore, be added to the list already given, so that, at the present time, there are four distinct varieties of *filaria sanguinis hominis*.

The steps by which our present knowledge of the *filaria nocturna* has been obtained were gradual. The embryo was first discovered by Demarquay in 1863 in the liquid of a chylous hydrocele; next, in the blood by T. R. Lewis, of India, in 1872. In 1876, the adult parasite was found in a lymphatic abscess of the arm by Bancroft, of Brisbane, Australia, and is accordingly known to helminthologists as the *filaria Bancrofti*, this name having been assigned to it by Cobbold. Finally, our knowledge of the life-history of the parasite has been completed by the genius of Manson. We are irresistibly reminded of the analogous history of the discovery of trichiniasis, with which the names of Hilton, Paget, Owen, Leidy, and Zenker are associated.

In the United States filariasis can no longer be considered as an extremely rare disease, and it is probable that it is more prevalent in certain of our southern states than is suspected. Professor John Guitéras, of the University of Pennsylvania, was the first to demonstrate the existence of endemic parasitic chyluria in this country,[1] and de Saussure[2] of Charleston, has published the clinical histories of twenty-two cases of filariasis observed in Charleston, S. C., from 1886 to 1890. Two cases of filariasis indigenous to Virginia have been reported by Dr. R. M. Slaughter,[3] but in neither of them was the blood examined. In both there were hemato-chyluria and filariæ in the urine, and in one filariæ were found in the pus of an alveolar abscess. While I believe these cases of Dr. Slaughter to be genuine examples of filariasis, I cannot refrain from the criticism that the illustration accompanying his paper bears but a superficial resemblance to the embryonic *filaria nocturna*.

Another indigenous case is reported by Dr. C. W. Mastin[4] of Mobile, Ala., the patient being a young man, aged twenty-two, who had never been outside of Mobile and its immediate vicinity. In Mastin's case the filarious lesion was a chylous hydrocele. The filaria is also said to have been found by Weiss in the urine of a child that had never been out of Illinois.[5] I am by no means sure that I have collected all the reported cases of indigenous filariasis; in fact, I have made no attempt to do so. Sufficient, however, has been said to show that the disease is widespread and not confined to tropical and sub-tropical regions.

In this connection I may remark that the embryonic *filaria nocturna* is capable of great resistance to cold. My slides prepared in winter and kept in a cold room, showed the parasites active at the end of six or seven days; in fact one lived for ten days. Exposure for many hours to a freezing temperature does not kill them, as proved by one of the methods employed by Manson to demonstrate "filaria ecdysis." This consists in placing the slide containing the filariæ upon a block of ice over night, in order to cause a separation of hemoglobin from the red corpuscles. The effect of this degree of cold is to render the movements of the animal somewhat languid, but after withdrawal from the ice they become as active as before, and the embryos speedily escape from their sheaths.

Facts such as those stated seem to prove that nothing but time is needed for filariasis, the scourge of certain tropical countries, to become prevalent in our own, and demonstrate the vital importance of municipal filtration of our water-supply.

I have said little about treatment in the foregoing remarks because I do not believe there is any drug capable of destroying adult filariæ in the human system. Surgeon-major E. Laurie, of Hyderabad, reports two cases which he believed to have been promptly cured

* I would suggest that the last-mentioned parasite would be much more appropriately called *filaria Mansoni*,

1 *Medical News*, April 10, 1886.

2 *Medical News*, June 28, 1890.

3 *Medical News*, September 5, 1891.

4 *Annals of Surgery*, 1888, vol. 8, p. 320.

5 *American Text-Book of the Diseases of Children*, Starr.

by thymol,[6] the maximum dose being five grains twice daily. Walsh, of the general hospital of Calcutta, also reports success from the use of thymol. On the other hand, Crombie, of the same institution has given two hundred grains of thymol daily in one case, and forty-five grains daily in another, without producing any effect upon the worms. As the latter justly remarks, "thymol is so exceedingly insoluble that it is improbable that any appreciable quantity of it left the intestinal canal." [1]

I consider it a very fortunate circumstance that the case of Fanny B. came under my observation at a time when I was able to secure the co-operation of such an expert in photomicrography as Dr. Charles Lester Leonard, whose arduous work has been pursued in the Laboratory of Hygiene of the University of Pennsylvania. Dr. Leonard succeeded in obtaining *photographs of the living parasite*, and this is, I believe, the first time that the living filariæ have been photographed under a one-twelfth oil-immersion lens, or, so far as I am aware, under any. The representations are, therefore, absolutely accurate, and necessarily take precedence of any drawings of the living or photographs of the dead nematode. To show the fallacy of drawings, I may say that one artist pictured the worm with a long cilium waving from its head, while another was unable to see anything of the sort in the same specimen. I may say, in this connection, that I am inclined to believe in the existence of such cilia, although they are not shown in the photographs; unless they are indicated by the blur in photographs taken. The point I wish to emphasize is that nothing can be represented in the photographs that was not present at the time they were taken.

I may also say that I have been unable to detect the "cephalic armature" described by Manson, although the "pouting" movement of the head was plainly visible. In mentioning my inability to detect the cephalic spine, or fang, I have no intention to impugn the accuracy of Manson's description. I attribute my failure entirely to my deficient training in this line of research.

In conclusion, I wish to express my thanks to Dr. Anna M. Fullerton, the distinguished physician-in-charge of the Woman's Hospital of Philadelphia, and to her assistants, Drs. Blackburn and Carpenter, for their kind and skilful co-operation with me in my study of this interesting case.

Postscript.—I visited Fannie B. at her home on March 25, and found her in good condition. She informed me that since leaving the hospital, her baby's feces had been stained blue. She is taking ten grains of methylene-blue daily—two grains every three hours.

On March 28, the patient's milk was faintly stained blue. The treatment was continued.

On the evening of March 29 I visited the patient and prepared eleven slides, in every one of which I found filariæ in most active movement. She had now been taking methylene-blue since March 12. From the 12th until the 21st, the dose was two grains every three hours—sixteen grains per diem—and from the 21st until the 29th, it had been ten grains a day, in divided doses of two grains. The drug, in this case, has proved absolutely inert. The patient's urine was deeply stained, and her milk very slightly stained. The baby's fecal discharges were blue, and his urine also of a faint bluish tint. Filariæ of a very faint bluish tinge could be seen. The leukocytes were unstained. A few filaments of cotton on the different slides were stained of a faint bluish tint by the blood-plasma with which they were in contact.

From the foregoing it is manifest that my experience with methylene-blue in parasitic chyluria is entirely different from that of Austin Flint and Joseph N. Henry.[2]

The latter in his report to Flint says: "The effects of methylene-blue in this case were decided and prompt. After the administration of two grains every two hours during the day on March 5, the parasites were very few at eleven P. M.; the only two found were

6 *Lancet*, February 14, 1891.

1 *Lancet*, August 13, 1892.

2 *New York Medical Journal*, June 15, 1895.

deeply stained with blue, and their movements were extremely sluggish, the urine being clear but intensely blue. On the fourth and seventh days no parasites were found, although the treatment had been discontinued after the first day. On the eighth day the urine became milky, and on the night of the ninth day, filariæ were found in great number, but their movements were not very active. On the tenth day the treatment was resumed, and continued for five days. Three days after, the blood being examined at night, a very few motionless filariæ were observed. Since that time and up to the present writing, the urine has been normal and the patient has been restored to perfect health."

Admitting the disappearance of the parasites in Joseph N. Henry's case, I believe it to have been a mere coincidence and in no way related to the administration of methylene-blue. I have given this drug in larger doses than were used in the case reported by Flint, and for a much longer period, without the slightest effect upon the parasite.

DISCUSSION.

Dr. Jas. Tyson said that he had had several opportunities to study chyluria, but that he had examined blood from these cases only once. This opportunity occurred in the case of a patient in the German Hospital—a sailor. He was totally unsuccessful in a midnight visit in discovering filariæ, although careful efforts were made to secure a specimen. He believes that this experience is not unusual and that it is admitted that chyluria may occur in the absence of this parasite. Nor does it seem necessary, as all that is required is some obstruction of the lymph-vessels, and anything that does this suffices. One of the most interesting features in connection with the subject is that, in the case of the filaria nocturna, if the patient sleeps in the daytime, the filaria comes to the superficial vessels at that time; so that it seems after all to have nothing to do with night, but that the condition of sleep favors its presence. Therefore, the terms, *diurna* and *nocturna*, are, in a sense, not precisely correct. A term representing that the filaria has something to do with sleep, rather than night, would be more correct. Its presence in the superficial vessels seems to be due to some relaxed condition of the vessels, as the result of which their diameter is increased. At other times, the superficial vessels being contracted, the parasite is compelled to keep to the larger deep-seated vessels.

Dr. A. Stengel said that on examining the blood of Dr. Henry's case, he was particularly impressed by the fewness of the parasites; and he felt that they might easily be overlooked. The examination recalled to his mind a case in the practice of the President, which he examined a year or more before. This was an instance of chyluria, but the parasites were not found after careful search of a number of preparations of the blood and several specimens of the urine. Dr. Stengel had no doubt it was non-parasitic, but he could not but remark on the ease with which one might overlook the filariæ, in the light of what he had learned from Dr. Henry's case.

Dr. J. C. Wilson stated that in the case referred to by Dr. Stengel repeated examinations of the blood yielded negative results. The patient, a young girl, was a native of Pennsylvania, and had never travelled beyond the borders of the State. The chylous urine ceased to be severe and she made a satisfactory recovery.

TYPHOID FEVER AS A COMPLICATION AND A SEQUEL OF INFLUENZA.

J. M. ANDERS, M.D., PH.D.

[Read February 26, 1896.]

During the recent epidemics of influenza, the first of which occurred in the latter part of 1889, I observed cases of influenza that apparently developed into pure typhoid fever. Two instances of this sort I have reported elsewhere.[1] In my former paper a brief allusion was made to some statistics, which showed that out of 338 cases of influenza, six were complicated with typhoid fever. In one of these typhoid fever developed after an interval of normal temperature lasting twenty days. Three additional cases, two of which occurred in the practice of Dr. I. N. Snively, are reported herewith.

Reference is not had here to the rather numerous instances of influenza with marked intestinal symptoms that more or less strongly resemble typhoid fever. Da Costa has recently reported a case of the latter sort.[2] These can, as a rule, be easily separated from typical forms of typhoid fever. Thus, in Da Costa's case, the appearance of the tongue (dry, and red at the tip), the swollen belly, and the marked general debility, in the absence of localized inflammatory lesions, pointed to typhoid fever, but the presence of a markedly irregular temperature-record, on the one hand, and the absence of either enlargement of the spleen or the typhoid eruption, on the other hand, showed the case to be one of influenza of adynamic type.

The chief object of the present paper is to confirm the statement made elsewhere that typhoid fever may appear as a late complication of influenza, or may follow this disease.

Among other affections which are known to be associated with influenza, as complications in varying degrees of frequency, are pneumonia, both croupous and catarrhal (with which latter disease pleurisy is always, and purulent pericarditis rarely, combined); gastro-enteritis, cerebro-spinal meningitis, etc. Again, its most common sequelæ are pulmonary tuberculosis, peripheral neuritis, perineuritis, melancholia, mania and neuralgia.

Cases of influenza which are complicated by typhoid fever are confessedly difficult of correct diagnosis without careful bacteriologic studies. I do not doubt that their dual nature has often escaped recognition. At all events, they have not been added to the literature of the subject previous to the appearance of my own article, so far as I know.

No bacteriologic observations were undertaken, either in connection with the cases reported previously or those herein narrated; hence, the evidence to show the clinical association of these two diseases has been gained only by observation of the symptoms at the bedside.

CASE I.—E. S., aged twenty-two, male, a student, was suddenly taken ill

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[1] "A Statistical Study of Influenza, *Philadelphia Hospital Reports*, Vol. III.

[2] "Cases of Influenza Simulating Typhoid Fever and Cerebro-Spinal Meningitis," *University Medical Magazine*, Philadelphia, February, 1894.

February 20, 1895. There were repeated slight chills, followed immediately by fever; intense headache; pains in the muscles generally, and great languor. On the following day I was called to his boarding-house, where he had betaken himself to bed. He presented great prostration and intense headache, the pain being situated especially deep in the orbits and in the region of the occiput. The temperature was 102° F.; the pulse, 100 per minute; and there was great pain in the muscles of the limbs, the loins, and the intercostal spaces. On the following day, as a result of the action of a purgative, he experienced severe gastro-intestinal pains with marked diarrhea. The latter symptoms were soon controlled by the use of paregoric and intestinal antiseptics. There was now some sore throat, tickling of the pharynx, and considerable dyspnea, but not much cough; nor were there any abnormal physical signs referable to the chest. His spleen was at this time not enlarged. The diagnosis of influenza was readily made. The course of the case for the next five or six days also fully corroborated this view. During the latter period the intense headache persisted. There was the usual restlessness, together with utter muscular prostration, and the burning and boring muscular pains, which changed their seat in the manner so frequently observed in influenza. The fever continued and was of irregular type, and the pulse was increased in frequency proportionately with the fever. At the end of the latter period, or about the time that defervescence was expected, the evening temperature continued at 103° F., and the morning temperature ranged from 101° to 102° F. The spleen now was slightly enlarged, the abdomen somewhat tender, especially in the ileo-cecal region; and there was slight diarrhea, amounting to two or three soft, ochercolored stools daily. No new nervous symptoms had appeared, but the very severe headache showed little tendency to abate. The catarrhal symptoms, referable to the nose and throat, however, had completely subsided.

On March 3d, typhoid fever was suspected. On March 4th (the twelfth day of the illness) the temperature was still pursuing the same continued type. The spleen was found, on palpation, to be considerably enlarged, and there appeared two characteristic typhoid spots on the upper part of the abdomen. The rose-spots faded away in the usual length of time, and subsequently a fresh and more abundant crop put in an appearance. The case was now treated as one of typhoid fever, to wit: by allowing a liquid, nourishing diet, by the use of cool baths, and internally quinin and salol in combination.

Apart from the fever, no other symptom demanded therapeutic measures save the headache and neuro-muscular pains, the latter affecting especially the loins and lower intercostal spaces. While the physical signs were negative, the patient was unable to take a deep inhalation at any time during the attack without experiencing pleurodynic stitches. These pains subsided with the decline of the temperature, and the subsidence of the typhoid symptoms—events which began at the end of the fourth week of the illness. Defervescence by lysis, so characteristic of typhoid, followed.

For the notes of the two instances following I am greatly indebted to Dr. I. Newton Snively, in whose practice the cases occurred:

Case II.—"I was called February 28th, at four P. M., to see Mr. A.W., aged forty-two years, who reported that while at work (as a brass-finisher), one hour before my visit, he was suddenly seized with a chill of moderate severity, soon after which he began sneezing and coughing. He developed speedily severe headache, pains in the muscles of the limbs and trunk, and soreness all over the body. At the time of my visit his temperature was 103° F.; the pulse, 100; the respirations, 24. He had, thus early, a naso-pharyngeal and bronchial catarrh. The heart was acting strongly and regularly. The patient gave a history of having felt well up to within an hour of the chill. During the three succeeding days he was much depressed in spirits; he was restless and wakeful at night; and also complained of nausea and loss of appetite. The bowels were regular.

"Under the usual treatment for influenza he rapidly improved. The temperature fell to normal on the fourth day, while the cough and myalgic pains were now, also, completely relieved. Appetite was restored, and the patient was apparently in a comfortable condition, so that I withdrew, with the instructions to the family and patient to send for me in case convalescence was in any way interrupted.

"Two days later I was sent for in the afternoon to see the patient again. I now found him with a temperature of 100° F.; the pulse, 90; the respirations, 22; and complaining of pain and tenderness in the abdomen, bleeding from the nose, and occipital headache. On physical examination I found the spleen slightly enlarged, the abdomen tympanitic, the tongue heavily coated, the mouth dry, the pupils dilated, and the patient quite restless, but there was no delirium. The nurse reported six loose movements of the bowels during the previous twenty-four hours.

"From this time on the patient advanced rapidly into characteristic typhoid fever. The temperature-curve became very characteristic; the epistaxis grew worse and became so unmanageable that I plugged the anterior and posterior naries on March 9th. The typhoid eruption appeared extensively on the latter date; the diarrhea persisted; the heart became weaker in action and the sounds more feeble; the respirations hurried and there was a slight cough, with a few bronchial râles on auscultation. The expression of the patient grew very dull and heavy, and the cheeks were flushed. The tongue became very tremulous; it was very dry and brown, and sordes collected on the teeth. Headache became very severe, and there appeared moderate deafness, stupor, muttering delirium, picking at the bedclothing and imaginary objects.

"Dr. J. M. Anders saw the patient with me on March 9th and confirmed the diagnosis of typhoid fever. He also thought the case had primarily been one of influenza, followed by typhoid. I saw the patient on the morning of March 10th, when the nurse gave a rather favorable report, as the patient had slept more than on any previous night for a week; his pulse was dicrotic but regular, and marked 112; the temperature was 102.5° F. The patient was taking milk and whisky regularly and in proper quantities. Nose-bleed was controlled by plugs; the mouth was kept moist by placing a large flat sponge (moistened frequently) over it. One hour after my visit I was again summoned, and, upon entering the room, found the patient dead. The nurse, who was an experienced graduate, reported that the patient had suddenly stiffened in bed, and when she took his pulse she found that it had gone up to 160, and was irregular and very weak; and that the patient seemed to be in a faint. She administered ammonium carbonate and gave a hypodermic of strychnin and digitalis, but with no effect, as the patient died a few minutes later. One hour after death large quantities of blood passed from the bowels, thus showing that the patient had died of an internal hemorrhage. He had shown evidence of the hemorrhagic diathesis from the first inception of the typhoid fever. No autopsy was made."

Case III.—"Mr. J. L., aged twenty years, a Canadian by birth, sent for me February 23, 1895. He complained of severe pain in the retro-sternal region, and sharp myalgic pains about the chest. He had a severe cough, paroxysmal and painful in character; he also had acute coryza; he expectorated a scanty, tenacious mucus; his temperature was 102° F.; the pulse, 96; the respirations, 23. On physical examination, I found a few irregularly distributed subcrepitant râles. The respiratory murmur was feeble. The tongue was coated; the appetite poor; the bowels regular, and the abdomen normal. The man had severe headache, and was very restless at night, but not delirious. He was very weak and perspired profusely. He told me that he had been working regularly every day, and felt well until this attack came on. He was employed as a stone-carver, and, at the time he was taken sick, was working on the outside at the Bourse building. I saw him every day for four days, when his catarrhal symptoms, fever, and cough sub-

sided. He now sat up out of bed and felt comparatively well, except that he was very weak and languid.

"On March 3, five days after I had made my last visit, I was again summoned to the patient with the report that he was suffering with severe abdominal pains, diarrhea, and vomiting.

"I found him with a heavily coated tongue; complete anorexia; a distended and tender abdomen; the temperature, 101° F.; the pulse, 90; the respirations, 20. I ordered a mixture of salol and bismuth, and put him on a liquid diet. From this time on he passed through a characteristic attack of typhoid fever; the temperature was higher each day than on the preceding, at first, and always higher in the evening than in the morning. The small, slightly elevated, rose-colored spots appeared on the abdomen on the 10th of March, and the spleen was manifestly enlarged. He had the characteristic nervous symptoms—headache, stupor, twitching of tendons, etc. The urine was small in quantity and very slightly albuminous. Convalescence set in after an illness of twenty-eight days, and was marked by falling out of the hair, anemia, etc."

In my present and previous articles are contained the notes of five cases in which typhoid fever was either a complication or a sequel of influenza. It cannot, in view of the statistics before quoted, be claimed with justice that typhoid fever is a frequent complication or sequel of influenza. That the epidemic prevalence of influenza is attended with an increased number of cases of typhoid fever, however, is also shown by tracings. I am of the opinion that further observation of influenza, with special reference to its association with typhoid fever, will confirm the dictum that the occurrence of the former disease renders the body more than ordinarily receptive to the typhoid bacillus.

DISCUSSION.

Dr. J. C. Wilson said that the paper was interesting as bearing upon certain facts relating both to influenza and enteric fever. He did not recall the two cases published by Dr. Anders in the previous communication upon the subject, but in the notes that were read of the cases it was very evident that influenza occurred as an intercurrent infection during the period of incubation of enteric fever. The latter is a fever of comparatively long incubation, ranging about two weeks or two and one-half weeks. The period of incubation of influenza is very brief, ranging from a few hours to a day or two. The whole course of the attack of influenza would appear to have exhausted itself prior to the appearance of the evidences of enteric fever. It is very interesting, indeed, that the subject should have been presented in this way. Dr. Wilson did not think that we can concur in the assumption of Dr. Anders from these cases, that influenza acts as a predisposing influence to enteric fever, because in the cases referred to influenza developed and ran its course during the period of incubation of enteric fever. In a community such as Philadelphia, in which we have annually four or five thousand cases of enteric fever, there must, during a pandemic of influenza, be very frequently instances of double infection, and this double infection will explain many of the cases of enteric fever that appear to begin abruptly with shivering, headache, and pains in the bones, which mark the onset of many of the infections, but not that of enteric fever. The Society is much indebted to Professor Anders for the presentation of the cases and his suggestive remarks with them, but the coincidence of the two diseases in the same individual does not prove that one predisposes to the other.

Dr. S. Solis-Cohen said that the paper and the subject are both extremely interesting. He once thought that it was well known and commonly taught that typhoid fever might appear during the course of influenza, or during convalescence therefrom, or that during the course of convalescence from typhoid fever symptoms of influenza might appear; and in a paper published in 1887, in the Medical and Surgical Reporter, he had referred to these occurrences as being within his own experience. He fully agreed with Dr. Wilson that the paper and the cases do not bear out the assumption that influenza and typhoid fever may be properly referred to as complicating conditions or one as being a sequel to the other, except in the matter of time; that is to say they occur together or in sequence without any essential or necessary etiologic or pathologic connection. Referring to details, Dr. Cohen recalled attention to the frequency of hemorrhage, as noted in the paper. Hemor-

rhage, especially from the nose, is a common feature of influenza. Hemorrhage from the bowel may likewise occur in influenza, and is one of the factors causing influenza to be mistakenly reported as typhoid fever. When typhoid fever occurs after influenza has developed, or when influenza attacks a patient in the course of typhoid fever the tendency to intestinal hemorrhage will be exaggerated. As in his own experience the number and the gravity of cases of this accident in enteric fever have been greater since the influenza pandemic of 1889 than before, he was inclined to attribute this in some measure to the lingering "influence" of influenza which does not always manifest itself frankly or is obscured by the predominating symptoms of enteric fever, but which, nevertheless, tends to increase the frequency and severity of intestinal hemorrhage. Dr. Cohen could not look upon this complication as a mere coincidence. The irregular fever and the violent headache and muscular pains, so much more frequent in enteric fever during recent years, may be attributed to the same cause; and Dr. Anders has done the profession a service in thus prominently calling attention to the association of influenza and typhoid fever.

THREE CASES OF PLASTIC NASAL SURGERY—FOR SADDLE-SHAPED NOSE, REMOVAL OF ENTIRE NOSE AND ARCHED ROMAN NOSE.

W. W. KEEN, M.D.

[Read February 26, 1896.]

CASE I.—SADDLE-SHAPED NOSE REMEDIED BY THE PERMANENT INSERTION OF A SILVER-GILT PLATE.

On April 9, 1895, A. McG., a woman of twenty-five, was admitted to the Jefferson Medical College Hospital, for treatment of a deformed nose. Eighteen years previously she had fallen and fractured her nose. The bridge of the nose was almost entirely obliterated, being scarcely more than an eighth of an inch above the level of the cheek, thus tipping her nose markedly upward and also making her mouth very oblique. I was consulted as to the possibility of remedying the deformity, and Dr. Walter J. Freeman kindly examined the nose and found that there was no impairment of either breathing or smell. I decided not to interfere in any way with the bones, but to carry out the following plan: Dr. W. J. Hall, a skilful dentist, who has paid special attention to the cure of such deformities, first took a cast of the nose, and upon this built up a wax nose which was satisfactory to the patient, as well as to me. From this as a model he constructed an artificial bridge, consisting of two plates of silver soldered together; the posterior plate corresponding to the shape of the bones, the anterior plate (somewhat smaller) corresponding to the desired outline of the remodeled nose; the space between the two plates was left vacant, but was entirely closed by soldering them together. This made the piece to be inserted much lighter than it otherwise would have been. All around the margin a row of small holes was drilled, which, it had occurred to me, would fix the plate firmly in place by granulation-tissue pushing through the apertures and being probably later transformed into fibrous tissue. The plate was then polished and heavily gold-plated.

On April 19, 1895, a transverse incission was made just above the alæ of the nose, and the superficial tissues loosened as far as the lower border of the frontal bone on each side, and slightly toward the tip, below the transverse incision. My object in loosening the soft parts towards the tip of the nose was to prevent the edge of the plate from protruding. The gilded plate was then inserted, and the opening closed by Halsted's subcuticular suture. The suture was removed six days after its insertion. The highest temperature was 99.4°. A bleb formed over the bridge of the nose, but soon healed.

The gold plate has never caused the slightest inconvenience, and on seizing the nose by the thumb and finger, the plate seems to be very firmly fixed in place.

Up to the night before the operation, I had believed that my idea was entirely original, but on that evening Dr. Taylor pointed out to me a paper in the London *Lancet* for February 17, 1894, by Dr. S. K. Ellison, of Adelaide, South Australia, reporting the insertion of a nasal plate, which, from the description, I am led to

believe was practically identical with my own. His operation was done in 1886, and after seven years the plate was still retained with entire comfort.

In a similar case I should be inclined to make an incision on one side of the nose sufficiently large to allow of insertion of the plate, with a very small incision upon the other side for the purpose of loosening the tissues from the bony and cartilaginous nose.

Since this operation was done, I have seen the notes of several somewhat similar cases. In the *Medical Record* of April 27, 1895, Dr. Weir reports the treatment of a case with a celluloid plate. He first attempted to use gutta percha, which proved unsatisfactory by reason of softening, and sometimes also was not tolerated. Later he hit upon celluloid, which had been used for closing apertures in the skull by Fraenkel, and this seems to have been very satisfactory. Attempts at inserting such plates through the nostril, which I also had considered, do not seem, as a rule, to be tolerated so well, as in two cases Weir was compelled to remove the celluloid plate, which lacked sufficient support from below. In two of his cases, through an incision in the cartilage within the nose, he chiseled out the bone and also removed a portion of the cartilage, thus restoring the contour. In another case he made a V-shaped incision inside of the mouth to bring down the lip. It will be noticed that in my own case the effect of raising the skin over the bridge of the nose has been to depress the tip of the nose and the upper lip, making what was a very oblique mouth a horizontal one and thus improving the appearance very much.

In the *Annals of Surgery* for June, 1895, Stimson also reports the case of a man, of twenty-five, who had a saddle-shaped nose from fracture. A piece of aluminum five-eighths of an inch long was inserted between the skin and the bones through a small incision in the alæ. As the outline of the nose was not satisfactory, a year later the wound was reopened, the piece of aluminum removed, and in its stead one of gutta percha, about half as large again, was inserted. The gutta percha in this case seems to have done very well.

In the *New York Medical Journal* of September 28, 1895, Foote has reported a case operated on by Bull, of New York, by means of a platinum bridge. In this case the upper lip and the nose were separated from the superior maxillary so as to expose the nasal bones. Holes were drilled into the left nasal bone and both superior maxillæ just outside the anterior nares. The platinum bridge had three supports, which were pressed into these holes. A steel pin was thrust through the nose from side to side, holding it securely in position. A considerable amount of swelling and headache followed the operation, but disappeared within a week. The steel pin was removed on the seventh day, and the patient left the hospital in seventeen days. The improvement was very great, but the outline of the nose does not seem to me to be quite as satisfactory as in my own case. There is apparently, however, the same improvement in the position of the mouth from the lowering of the tip of the nose. It seems to me that the multiple holes in the side of the plate are quite sufficient to hold it in place, if I may judge by the result of my single case, instead of the more severe operation resorted to by Bull.

Schimmelbusch (*Archiv für Klinische Chirurgie*, 1895, p. 739) condemns the use of all foreign bodies, including gold-plate bridges, ivory, etc., but the cases which I have cited in this paper show that he is quite wrong in his conclusions against them. In fact, I am by no means sure but that in the second case I might have been able to construct a new nose by a forehead flap, giving it shape by means of such a gilt-silver support.

CASE II.—REMOVAL OF THE ENTIRE NOSE FOR SARCOMA, WITH AN ARTIFICIAL NOSE FIRST OF ALUMINUM AND LATER OF SILVER.

W. J. L., aged fifty, was kindly sent to me at the Jefferson Medical College Hospital, December 3, 1894, by Dr. T. A. Enos, of Townsend, Del. The personal and family history was negative. About four years previous to coming

under my care the patient had noticed a small wart on the left side of his nose. Two years later he tried various quack cures for it, when it began to grow, and when I saw him it was a circular tumor, one and one-half inches in diameter, extending from a little to the right of the bridge of the nose slightly on the left cheek. The skin of the right side of the nose was infiltrated, and also that of the left cheek to some distance from the growth.

On December 5, 1894, by knife, bone-forceps, and chisel, the entire nose, excepting a little bit of each ala and the septum, was removed, together with a small portion of the adjacent left cheek, the skin of which was involved. The incision just grazed the inner angle of the left eye. The left antrum was opened slightly. The growth was found to have penetrated deeply into the nostril. The hemorrhage was moderately severe, but easily checked by the Paquelin cautery, hot water, and pressure. The entire wound was then packed with iodoform-gauze, and dressed as usual. The wound was sufficiently healed for him to go home on the first of January, 1895.

The question arose then as to a new nose, and I decided not to do a plastic operation, but to have an artificial nose applied, because, *first*, not only was the nose removed, but a considerable part of the left cheek also; this would require so large a flap as to take a large part of the skin of the forehead and leave a very unsightly scar. *Secondly*, so broad a flap would not retain any prominence like a normal nose, but would flatten out as a mere flap of skin. And *thirdly*, I feared that so large a flap would probably undergo more or less gangrene.

Accordingly, I again invoked the kindly services of Dr. Hall, who constructed an artificial nose of aluminum, so as to be very light. It was constructed with a flange below, which hooked behind the bone, and at the upper end with a short lever actuated by a spring. The lever could be pulled down by a wire which was scarcely visible where it protruded through the artificial nostril. When the lever was pulled down by this wire, it could be pushed behind the projecting bone at the top of the large opening, and on releasing the lever the spring pushed it up just behind the bone and so held the nose in place without spectacles or other artificial means.

The lightness of the material from which it was constructed made the pressure both at the upper and lower borders so slight as to give no trouble. A very unexpected difficulty, however, presented itself after the lapse of between two and three months. The nose came entirely to pieces by the destructive action of the tears on the soft solder—hard solder cannot be used with aluminum. The metallic wire which pulled down the lever was made of equal parts of aluminum and tin, and this also was dissolved away. Dr. Hall, accordingly, made a second nose of coin-silver, which has answered very much better.

The new nose was painted to resemble a flesh-color as nearly as possible, and improved the patient's appearance very much, the large hole left by the healing of the parts being most unsightly. The disease so far has shown no tendency to recur.

CASE III.—A MARKEDLY ARCHED ROMAN NOSE CONVERTED INTO A STRAIGHT GRECIAN NOSE.

E. H., aged thirty, entered the Jefferson Hospital March 26, 1896. Six years ago he sustained an injury to the nose which rendered a congenital prominence still more marked. The nose, was markedly arched, though no more so than many noses whose owners are quite satisfied with them. He stated to me that its appearance interfered seriously oftentimes with his obtaining employment as a waiter, and that he wished it made into a straight or Grecian nose, though he did not express it in these words; I tried to dissuade him from operation, as his nose was a very respectable one, but he insisted very strongly upon it.

Operation was performed March 26, 1896. A linear incision was made to the bone from near the tip to the base of the nose, and the soft parts dissected loose on each side. With a drill I then penetrated the bones of the nose about the middle of their length.

This drill did not go through the soft parts, which were first retracted and then held out of my way by the drill which I left in place for the time being. I then chiseled out a piece of this shape, (), about twenty-two millimeters long and at its widest part five millimeters wide. This made an opening into the interior of the nose. I then attempted with a pair of strong forceps to crush the sides of the nose together, but found they were still very resistant. One or two blows of the hammer on a chisel just at the juncture of the nose with the cheek fractured the two sides of the nose. I was then able to bring them together very well. I then withdrew the drill and by means of a straight needle passed a stout thread of catgut through and tied it tightly. This held the bones in place. A needle threaded with another thread of catgut was passed through the cartilage, which had been partially removed, and the tightening of this ligature brought the whole of it together very nicely. A little trimming of the surfaces was needed with the double rongeur forceps, and then I closed the incision in the skin with Halsted's subcuticular suture. The result was most satisfactory as regards shape, there being a good straight nose instead of the curved nose which the patient originally had.

He made a speedy recovery, without fever or suppuration, and was discharged six days after the operation entirely cured. The scar is scarcely visible.

Dr. G. G. Davis exhibited

"A New Needle-Holder."

Desiring a needle-holder for Hagedorn and other needles, that was less clumsy and less complicated than the ordinary forms, the following was devised: It consists of three pieces, the two blades and a connecting rod or lever. The lower blade is stiff and rigid throughout; the upper blade bends when pressure is made. When the handles are closed, the lower blade is pushed forward by means of the connecting lever. To take the holder apart, the button on the under side of the upper blade is turned with the fingers and the lever allowed to drop. The under blade and lever are then slid out of the upper blade. To separate the lever it should be raised at right angles to the blade and unhooked. If a catch is desired, one is formed by continuing the lever down across the opposite handle. Personally, Dr. Davis dislikes a catch and does not use it. Some of the advantages of the instrument are the small size of the jaws; the fewness of its parts—three; and the ease with which it can be taken apart for cleansing.

A CASE OF DUBOISIN-POISONING.

C. A. VEASEY, M.D.

[Read March 11, 1896.]

Cases in which there exists an intolerance of duboisin, either when administered internally or when solutions are instilled into the conjunctival cul-de-sac, are not of such rare occurrence as to make it desirable that each new case should be reported. As early as 1879 Nettleship[1] reported eight cases of poisoning from the instillation of several drops of a solution of this drug of the strength of four grains to the fluid-ounce, the symptoms varying from slight transient giddiness to violent delirium. In the same year a case was reported by Carl[2] from the instillation into each eye of three or four drops of a solution of the same strength, and another by Tweedy,[3] the latter case being one of iritis, and the drug being employed to break up the synechiæ, as it was supposed to be much stronger in its action than atropin. Later, cases were reported by Chadwick,[4] Kollock[5] and others, but in all of the cases referred to the solution was of the strength of four grains to the ounce and in most of them several instillations were made before symptoms of poisoning were observed.

In the present case, however, the quantity of the drug giving rise to severe symptoms was much smaller, and the rapidity with which these symptoms appeared was so marked that they give to the case additional interest.

1 *Lancet*, 1879, ii, p. 352.
2 *Klin. Monatsbl. f. Augenheilk*, 1879, xvii, p. 337.
3 *Lancet*, 1879, ii, p. 441.
4 *Brit. Med. Jour.*, 1887, i, p. 327.
5 *Medical News*, 1887, i, p. 344.

Miss P——, aged twenty-seven years, a stenographer by occupation, consulted me in 1892 for constant headache, always much worse in the afternoons and towards the close of the day and never so bad on the days on which she was not obliged to work. There were also symptoms of accommodative asthenopia, some ciliary pain, and slight conjunctivitis, all of which were made worse by close application to her work. Her general health was very good beyond the strain and annoyance caused by her ocular symptoms.

Upon examination there was found to exist compound hyperopic astigmatism, the axes being 135° and 45°, and this was corrected with the aid of cumulative instillations of homatropin hydrobromate as the cycloplegic. All of the eye-symptoms rapidly disappeared and she was able to perform her work with absolute comfort.

In 1895, a little more than three years from the time she had been glassed, she consulted me at my office, saying that she had accidentally broken her lenses, and inasmuch as some of the old symptoms had returned about a month before, she thought perhaps she might require some change. After making the preliminary examination, she was directed to employ cumulative instillations of homatropin hydrobromate, as before; but, with a trial of two different solutions obtained from two different sources, it was found impossible to relax all of her accommodation. A solution of duboisin sulphate was then ordered, of the strength of two grains to

the fluidounce, of which she was to instil one drop into each eye, three times a day.

The patient resided in one of our suburban towns, and, not understanding that it was necessary for her to use the drops on the day previous to the one upon which she was to come to my office, did not have any instillations made until she was ready to leave home on her way to the city to be refracted. On the train she felt somewhat heated and flushed, and, to use her own expression, her "brain felt as if too much champagne had been taken," she was so excited, and when she reached the station in the city, she found it almost impossible to walk. Taking a carriage, she was driven at once to my office, arriving there in about forty minutes from the time the drops were put into the eyes. She staggered into the reception-room, talking in an incoherent manner to my door-attendant, so that it was at once reported to me that an intoxicated woman was in the office. I saw her immediately, and, suspecting that either the duboisin or hysteria was the cause of the symptoms, immediately placed her on a couch in my library. In going from the office to the library it was ascertained that she could not walk without assistance, and that her gait was of the dragging character seen in certain lesions of the spinal cord. She was intensely excited, talking in an incoherent manner and replying to all questions vaguely. Occasionally a proper reply to a question could be obtained, but most of the answers indicated delirium. The throat was exceedingly dry and the pupils moderately, but not fully, dilated. The face and hands were flushed, presenting an appearance indeed not unlike a scarlatinous eruption; the pulse was small but strong and rapid, the number of beats a minute being 124; and the respirations shallow and hurried, being twenty-eight a minute. In half an hour the patient lost almost all control over herself, not being able to raise her head, arms or legs without assistance, though she was able at times to move her fingers. In regard to the latter movements, however, I was not sure that they were not reflex. The flushed condition was now disappearing and she was becoming pale in spots; the speech was reduced to a whisper now and then, and no proper replies to questions could be obtained. The pulse had become soft and compressible, though but slightly reduced in frequency, and the respirations were now fourteen in a minute and still shallow. There seemed to be partial anesthesia of the whole body.

The symptoms described lasted for about three hours, when they began to disappear gradually. When control of the voice began to return there was low, muttering delirium, lasting about half an hour before full control was gained. In four hours from the time the patient had arrived at my office she was able to take a carriage and go to the house of a friend, after having given me a most uncomfortable morning.

On the following day she returned, accompanied by her sister, who had made the instillation of the solution on the preceding day and by whom I was assured that only one drop was placed in each eye, as I had cautioned the patient that the drug was poisonous and must be used very carefully. The probability of hysteria being the cause of the symptoms was excluded by correcting the refractive error with the use of atropin sulphate as the cycloplegic, and beginning its use at once, two instillations being first made at my office to ascertain if there was any systemic effect. I had the solution of duboisin sulphate examined and it contained only the quantity called for in the prescription, namely one grain to the half-ounce of distilled water.

It is impossible to say accurately just how much of the drug was absorbed into the system. The point of the dropper was a very small one, so that it is probable that about $\frac{1}{240}$ of a grain was placed in contact with the conjunctiva. It is, however, impossible that all of this was absorbed.

The case is interesting and instructive in showing that so small a dose of duboisin may give rise to such alarming symptoms.

DISCUSSION.

DR. H. F. HANSELL expressed his interest in this subject, as he uses the drug constantly in patients under forty years of age, and has had some experiences much like those related. Lately, however, they have been fewer, he thought, because of the different sources from which the drug is obtained. The composition of the little vials of duboisin is not always the same. Some druggists will put up a solution which is practically without sensation to the conjunctiva; others one that produces severe burning. In some cases it takes considerable time for the drug to act; in others it acts at once; so that there must be different preparations in the market. The amount that comes in contact with the conjunctiva does not produce the ill-effects, but that which is absorbed through the nasal cavities and throat.

DR. G. E. DE SCHWEINITZ said that Dr. Veasey's paper is an excellent report of the toxic symptoms that not uncommonly follow the use of duboisin as a mydriatic. Having had one or two disagreeable experiences with the drug Dr. de Schweinitz rarely uses it, although it is sometimes employed in his hospital practice. Occasionally, in private practice, he gives it to medical students, chiefly because it paralyzes the ciliary muscle as thoroughly as any drug with which he is acquainted, and it possesses the advantage of being a cycloplegic agent of short duration. The paraplegia that Dr. Veasey has described is often marked, and is to be attributed to some influence of the drug on the motor centers of the cord. The medicament is much used in the treatment of insanity. No doubt many of these cases are to be explained by idiosyncrasy on the part of the patient, and perhaps also, as Dr. Hansell has suggested, by different strengths or actions of the alkaloid as derived from different sources. The most alarming case of mydriatic poisoning which has ever occurred in Dr. de Schweinitz' practice resulted from the instillation into the conjunctival sac of a solution supposed to contain one grain of homatropin hydrobromate in a dram of water every fifteen minutes for an hour and a half. The symptoms suggested hyoscyamin-poisoning, and an analysis of the solution proved that this drug had by mistake been substituted for the homatropin which was ordered.

DR. FRED'K HERBERT said that he always mixes his own mydriatic and thus avoids any such trouble as has been mentioned.

CONCERNING THE REPAIR OF CORNEO-SCLERAL WOUNDS, WITH PROLAPSE OF THE IRIS.

G. E. DE SCHWEINITZ, M.D.

[Read March 11, 1896.]

For practical purposes, prolapse of the iris, occurring at the corneo-scleral junction, or its immediate neighborhood, may be divided into those varieties which result from a perforating ulcer and those which follow a wound, either accidentally inflicted or designedly placed, as, for example, in the corneal section of cataract-extraction.

The treatment naturally consists of two procedures: non-operative, *i. e.*, the use of eserin and a pressure-bandage; and operative, viz., abscission of the prolapse and closure of the wound. It is to the best method of dealing with these cases from the operative standpoint that I desire to call attention, in the hope of eliciting some discussion from my colleagues present to-night.

First, the method of Gama Pinto for obtaining a non-adherent cicatrix.

As is well-known, this surgeon abscises the prolapsed portion of the iris, frees all adhesions to the margin of the ulcer, and covers the opening in the cornea with a flap of bulbar conjunctiva, which should be cut twice as large as the opening and pushed into the orifice with a blunt probe. A firm binocular bandage is applied and the eye not dressed until the third day. Then it will often be found that the conjunctival flap has healed into the ulcer. A flat, non-adherent cicatrix results, or, in other words, an ordinary corneal scar without staphylomatous bulging, and a circular pupil.

I have employed this method several times and with gratifying success, although I have not always been able to secure non-adherence of the iris to the cicatrix.

For example, a patient now in the Philadelphia Hospital was admitted several years ago with a large marginal ulcer occupying the entire upper and outer portion of the cornea, which had perforated in one corner and permitted the prolapse of a large portion of the iris. This was abscised in the usual way, and the Gama Pinto directions followed. It was an unfavorable case, owing to the extent of the ulcer and the shape and character of the opening, which followed the curve of the cornea for some distance. At present, fully six years after the accident, the point of prolapse is occupied by a perfectly flat white cicatrix, to which there is slight adherence of the iris, so that the pupil is drawn upward and outward. The vision of the eye is excellent and the patient has no trouble with it, being able to sew the entire day—a result, considering the extent of the ulceration and prolapse, far better than was to be anticipated.

In another case, which I have recorded briefly in the *Philadelphia Polyclinic*, the patient suffered from monolateral gonorrheal conjunctivitis, complicated with a sloughing ulcer at the inferior portion of the cornea, perforation, and a large prolapse of the iris. The iris was abscised, the margins of the aperture carefully cleansed, and the iris freed as much as possible. The opening was then closed with a flap of conjunc-

tiva *transplanted from the other eye*, the size of the flap being almost that of the circumference of the cornea, or, in other words, fully three times the size of the original opening. The result was admirable; the graft became adherent, there was no bulging, or very little protrusion of the cicatrix, and, although the iris was somewhat adherent and the pupil distorted, vision was about one-seventh of normal — far better than could possibly have been expected from the serious nature of the lesion and the extensive prolapse of the iris. The man returned after nearly three months, and the following note was made: Graft covering lower third of cornea. Vessels from conjunctiva pass over graft. Pupil partially covered. Vision, fingers at two meters, without correction.

Should the method of Gama Pinto fail to secure a non-adherent cicatrix after the healing had become firm, there would be no objection to the performance of Mr. Lang's operation of dividing the anterior synechiæ with a blunt knife-needle—a procedure that I have practised in a number of instances with almost universal success.

The advantage, then, of the Pinto flap, even if non-adherence of the iris is not secured, consists in a more rapid healing of the corneal wound, together with the prevention of staphylomatous bulging.

Second, the closure of the wound with stitches.

The report of a few cases will illustrate this method:

Case I.—A male, aged eighteen years, was admitted to the wards of the Philadelphia Hospital on May 11, 1895, with violent bilateral gonorrheal conjunctivitis. The right cornea was still clear; the left cornea had already begun to be opaque.

In spite of treatment there rapidly formed on the left side a large corneal abscess, and on the right side, nine days after admission, a large crescentic ulcer formed at the upper and outer corneo-scleral junction, which speedily perforated, permitting a huge prolapse of the iris. It was impossible to make pressure, owing to the inflamed conjunctiva; eserin was tried without effect, the prolapse and staphylomatous bulging becoming greater every day. Therefore, a week after the appearance of this prolapse it was abscised (the discharge from the conjunctiva had almost ceased, although the membrane was still vascular), the edges of the wound freed as much as possible, and the iris replaced with a spatula. The corneal and the scleral edges of the wound (if I may so express myself), which occupied the upper and outer third of the corneal rim, were now freshened and united with four sutures. The sutures were removed on the fourth day, and the wound found firmly united.

When my term of service ended, six weeks later, vision was $\frac{20}{30}$ without correction, the media clear, the pupil slightly oval and drawn upward and outward, and the eye quiet, although there was still some thickening of the papillary layer of the conjunctiva, owing to the previous conjunctivitis. The vision of the left eye was counting fingers, owing to the presence of a central corneal macula.

Although not bearing upon this topic, it is interesting to note that this patient during his attack of gonorrheal conjunctivitis successively developed synovitis of all the large joints, beginning with the left knee. With each attack of synovitis there was marked exacerbation of the corneal symptoms, and it was during the attack of synovitis in the left knee-joint that the perforating ulcer which I have described appeared. I have never before seen so extensive a case of gonorrheal synovitis.

Case II.—A female child, aged twelve years, came for treatment to the Jefferson Medical College Hospital on December 27, 1895, presenting at that time, according to the records, a phlyctenular kerato-conjunctivitis, with an ulcer at the inferior margin of the cornea. This ulcer must have gone on to perforation, and when I came on duty, on the 13th of January of the present year, there was a large prolapse of the iris, with beginning staphylomatous bulging. On the 17th of the same month the prolapsed iris was excised, the edges of the wound freshened and closed with a silk suture, after replacement of the iris.

The suture was removed on the third day, and recovery has been uninterrupted; the eye, previously irritable and congested, rapidly became white and quiet, and now the opening is closed by a firm white cicatrix, without bulging, the pupil is nearly circular, although there is some attachment of the iris below, and vision with the best correcting glass is $\frac{20}{40}$.

Case III.—Kate Ingram, aged fifty, an insane patient in the Philadelphia Hospital, was admitted to the Ophthalmic Wards with double cataract. Both lenses were extracted without iridectomy. In the right eye there was kind healing, without accident; in the left eye a large prolapse of the iris was found twenty-four hours after extraction. The latter was treated in the usual method by the instillation of eserin and a compress bandage, and somewhat lessened in size.

Three weeks after extraction the prolapse was abscised, the iris replaced as much as possible, and the wound closed with two silk sutures, which were removed on the fourth day. The healing was perfectly kind; the pupil, instead of being round, is a vertical oval, and vision on February 1st, after the correction of six diopters of astigmatism, was $\frac{6}{20}$. This astigmatism will very much decrease in the course of time, and no doubt there will be corresponding improvement in vision.

I shall not occupy more time with additional clinical histories, as these are sufficient to illustrate the class of cases to which this procedure is suited, namely, wounds and ulcers at or near the corneo-scleral junction, associated with prolapse of the iris, in which, after removal of the prolapsed iris, it is possible to secure perfectly clean corneal wound-edges. I would prefer not to pass a stitch if the margin of the wound was infiltrated or gray. I would also hesitate about passing a stitch if a wound at the same time had injured the ciliary body.

It seems to me that the method of Gama Pinto is preferable if the opening is distinctly circular, and if it is not possible to obtain a perfectly non-infiltrated wound-edge without destroying too much corneal tissue, as, for example, in the case of monolateral gonorrheal conjunctivitis, with perforating ulcer. If, after abscission of the prolapsed iris, the wound is elliptical, or follows the curve of the cornea, as, for example, in Case I, stitches seem preferable to a graft.

I have as yet not excised the ulcerated tissue of a large corneal ulcer and then stitched the edges of the wound, but I am inclined to try the measure. Neither have I stitched wounds situated at some distance from the edge of the cornea.

Corneal sutures are inserted as a rule by some cataract-operators after simple extraction, for example, by Kalt, in France, who is a strong advocate of this procedure. So, also, a number of operators have advocated the insertion of these sutures after the abscission of a prolapsed iris subsequent to simple extraction.

The point of this matter evidently is that we are able to insert delicate sutures in the cornea with more impunity than we would be led to believe from the ordinary text-book descriptions of these lesions. I have always used delicate silk thread and the curved needle which comes in Dr. Stevens' tenotomy-case. I have not tried catgut, and do not believe that it would be as satisfactory as the silk.

DISCUSSION.

DR. H. F. HANSELL said that he was not at all prepared to discuss this subject, from his own experience; because he had never sutured the cornea. There is no reason, however, in his opinion, why sutures should not be introduced into the cornea, as well as in other structures, with perfect safety. In most cases in which corneal sutures could be applied, it happens that the eye is so badly damaged by the purulent process or by the accident, that vision is irretrievably lost. The iris is inflamed, the lens frequently escapes, a false membrane obscures the pupil and atrophy results. The cases reported by Dr. de Schweinitz are interesting, as they have been followed to their conclusion. Dr. Hansell's experience has been limited to transplanting the conjunctiva. This operation is popular and is usually successful. By transplanting from the eye of an animal, or from another eye, or moving the conjunctiva up over the eye that is damaged, the injury to the cornea may almost always be covered up, without materially altering the rotatory movement of the eye.

DR. C. A. VEASEY said that his experience in treating corneo-scleral wounds has been principally obtained by assisting Dr. de Schweinitz in some of the cases he has had within the past three or four years; so that anything he might add would be only to testify to the remarkably good results that were obtained in most of them. Especially was this the case in the child with a large prolapse of the iris which had been increasing in size daily for several days and in which, after excision of the prolapse, the corneal opening was closed by sutures. Dr. Veasey had seen the child a few days before and all signs of irritation had subsided; the eye was perfectly quiet and the girl was attending to her school-duties.

DR. DE SCHWEINITZ agreed with Dr. Hansell that there is no good reason why we should not insert stitches in the cornea, as well as in other portions of the body. In the cases that he has recorded the injuries to the deeper structures were not such as to preclude the possibility of sutures being serviceable, and he was inclined to give this method of treatment a further trial.

CONCERNING THE EXTRACTION OF IMMATURE CATARACT, WITH THE REPORT OF CASES.

G. E. DE SCHWEINITZ, M.D.

[Read March 11, 1896.]

Practically all ophthalmic surgeons are agreed that about the sixtieth year the lens may be extracted safely, even if it is in part unclouded, because, if Schweigger is correct, at that period of life the usual criteria of ripeness are erroneous, accommodation being annulled by physiologic changes in the lens. Whether we may with equal propriety extract immature cataract in younger individuals, is still a matter of dispute, and it is much to be desired that some rational and safe procedure may be devised to relieve the patient from the long period of semi-blindness, which often exists while a senile cataract is slowly advancing to maturity. Not only is this stage present in senile cataracts, but also in other types of opacification of the crystalline lens, namely, zonular cataracts, immature traumatic cataracts, and the partially formed cataracts in young people.

Recently, this subject has been discussed at some length by Dr. John E. Weeks in a paper entitled "The Operative Treatment of Immature and Some Forms of Zonular Cataract," which was presented to the Baltimore meeting of the Ophthalmologic Section of the American Medical Association. Twenty-five cases, without a failure, were reported, and the result appeared to this writer to be as favorable as those ordinarily obtained by the removal of cataract at the stage of maturity.

It is interesting to note one or two of the comments on this paper. Knapp, for example, refers to the fact that the ripening operation unfortunately is often done without effect, often provokes iritis, and has even been followed by plastic and purulent irido-cyclitis. Therefore, if patients will not wait, he prefers the risk of dealing with the remnants by secondary discission to the double operation of ripening and extracting the lens. Hotz, of Chicago, describes his experiences with ripening processes as unsatisfactory, either because the lens did not ripen, or because his efforts were followed by cyclitic irritation, and concludes that the extraction of immature cataract does not involve greater danger than the removal of a fully mature cataract, provided there is not a very large amount of absolutely transparent lens-matter. When this is the case, the patient's vision is usually still so good that there exists no urgency for an operation. Not a little interesting are the remarks of Dr. White, of Richmond, who, as we all know, is a strong advocate of one form of ripening operation. He says, "I never ripen immature cataract if the case will wait for nature's processes, and then only if the patient is under sixty years of life, because I thereby break up and loosen the cortex from the capsule, simplifying the extraction; over sixty years of age, I extract without waiting for maturity."

First, concerning the ripening of cataracts.

I have taken the liberty of introducing this subject in the hope that it may

elicit discussion from my colleagues, and not in any sense to bring forward new views or new methods. My own experience with ripening consists in the performance of one of two operations: Either the Foerster method, or, in a few instances, the method which is particularly advocated by Boerne Bettmann, of Chicago, namely, direct massage of the lens-capsule. I have not performed either of the operations many times, and thus far have no bad result to record; perhaps because my experience is a limited one. I have, however, seen extremely bad results in the hands of others, not only from primarily induced iritic and cyclitic complications, but also when the ripening process proceeded smoothly, but at the extraction the capsule was found thickened, the cortex clinging tenaciously, and the toilet of the wound much more difficult than would have been the case had the cataract been extracted in its immaturity.

Second, concerning the method of extracting an unripe cataract.

Knapp, Weeks and other operators of skill and experience, employ the simple extraction, precisely as they would for an uncomplicated mature cataract. In all my cases of unripe cataract I have used the combined method, sometimes with a preliminary iridectomy, with the exception of one instance, in which I did simple extraction. I have preferred iridectomy because it has seemed to me that prolapse of the iris, from the danger of which the best operator is not free, added to the increased danger of cortical remnants, multiplied unnecessarily the risks of the patient, and, moreover, that the toilet of the wound was more easily accomplished. Perhaps with wider experience and increased skill, my opinion on this subject may change. Certainly the experience of Weeks indicates that simple extraction is satisfactory.

Third, concerning the opening of the capsule.

Theoretically, peripheral opening of the capsule, and, therefore, confining cortical remnants, is the proper procedure. Practically, with the exception of one case, I have always freely opened the capsule by a T-shaped flap, and, usually, immediately at the conclusion of the operation have instilled a drop of sterilized atropin - solution. I have avoided iritis, except in one instance. In this case, however, there was trauma during the process of healing.

Fourth, concerning irrigation of the anterior chamber.

I believe this to be a dangerous procedure and never employ it. Occasionally, in clearing the wound, I have made slight pressure on the sclera above the corneal section with a sterile spatula, and allowed my assistant to flood the wound-edges with a tepid boric-acid solution, or a sterile normal salt-solution, but I have never, in recent times, injected these fluids into the chamber. I am well aware that surgeons with large operative experience report most satisfactory results from irrigation of the anterior chamber, particularly Lippincott, of Pittsburg, who practises irrigation as a routine matter, but I am not inclined to burden the extraction of cataract with an additional manipulation when equally good results are obtainable without its use.

In the accompanying table I present twelve cases in which I have extracted cataract which was immature. Four of these were immature nuclear cataracts, six immature senile cataracts, one immature soft cataract, and one immature traumatic cataract.

The immature soft cataract was removed by simple extraction and the ultimate vision was $\frac{5}{5}$. In all of the others iridectomy was performed, in five of them preliminary iridectomy, four times by myself, and once by another surgeon. The recovery in all was uneventful, with the exception of one, in which a traumatism on the fourth day produced a hemorrhage into the anterior chamber, with subsequent iritis, the vision, however, being $\frac{6}{12}$, which an ultimate discission, not yet performed, will doubtless raise to a higher standard.

Two of the cases are interesting as presenting the extraction of immature cataract in high myopia, a myopia of not less than eleven diopters, and associated with extensive atrophic myopic choroiditis.

In six of the cases subsequent discis-

No.	Sex.	Age.	Health.	Cataract.	Date.	Operation.	Recovery.	Immediate Vision.	Secondary Operation.	Ultimate Vision.	Remarks.
1	M.	58	Chronic alcoholic.	Immature senile. V. = counting fingers.	1. 18. 90.	Preliminary iridectomy in March, 1889. Extraction 1. 18. 90. Some cortical remnants in coloboma.	Uneventful.	Counting fingers.	6. 6. 91, Three discissions	Counting fingers.	Small atrophic disc; shrunken arteries; patient able to get about; clear coloboma.
2	F.	40+	Feeble.	Immature nuclear. V. = fingers at 1 foot.	5. 17. 95.	Preliminary iridectomy, 12. 14. 94. Extraction, 5. 17. 95.	Uneventful; small prolapse of inner pillar of coloboma.	$\frac{6}{30}$; 23 days.	No discission.	$\frac{5}{30}$	Patient highly myopic; correcting lens + 1; many vitreous opacities; huge posterior staphyloma and myopic choroiditis.
3	M.	66	Good.	Immature senile. V. = fingers at 1 meter.	10. 4. 95.	Preliminary iridectomy in Brooklyn two years ago; extraction, 10. 4. 95; clean delivery of lens.	Uneventful.	$\frac{6}{30}$; 20 days.	Dec., 1895.	$\frac{6}{18}$	Old corneal macula; clear coloboma; a few slight vitreous opacities.
4	F.	75	Good.	Immature senile. V. = fingers at 6 inches.	11. 7. 95.	Combined extraction; clean removal of lens; thick capsule.	Traumatic hemorrhage into anterior chamber on fourth day; slight iritis.	$\frac{6}{18}$; 22 days.	Not yet performed.	$\frac{6}{12}$	Several posterior synechiæ; capsule thick.
5	M.	74	Good.	Immature senile.	11. 9. 95.	Combined extraction, 3 mm. flap; some cortex remaining.	Uneventful.	$\frac{6}{30}$; 12 days.	Declined.	$\frac{6}{30}$	Some cortex remaining in wound; patient left hospital 12 days after extraction; extraction in opposite eye of right cataract 2 weeks previously; V = $\frac{6}{12}$ without discission.
6	M.	58	Good.	Immature nuclear. V. = $\frac{2}{40}$.	11. 11. 95.	Preliminary iridectomy, 6. 10. 95; extraction of lens, 11. 11. 95; much cortex remaining.	Uneventful.	$\frac{5}{40}$	1. 6. 96.	$\frac{5}{10}$	Tension soft, thick capsule remaining, but with discission large clear space through web.
7	F.	40+	Feeble.	Immature nuclear. V. = counting fingers at 2 feet.	11. 20. 95.	Preliminary iridectomy, 6. 3. 95; extraction, 11. 20. 95.	Uneventful.	$\frac{6}{30}$; 33 days.	No discission.	$\frac{5}{30}$	High myopia; posterior staphyloma and extensive retino-choroiditis; vitreous opacities; is able to sew with + 4.50; several bands of capsule above and below, with clear space between.
8	M.	58	Good.	Immature nuclear. V. = counting fingers.	12. 2. 95.	Preliminary iridectomy, 6. 28. 95; lens extracted, 12. 2. 95.	Uneventful.	$\frac{5}{22}$; 18 days.	Jan., 1896.	$\frac{5}{10}$	Tension of the eye soft; two small bands of capsule remaining across pupil.
9	M.	27	Albuminuria.	Immature soft. V. = fingers at 1 meter.	12. 16. 95.	Simple extraction; Knapp's section.	Prolapse of iris; restoration under eserin on sixth day.	$\frac{5}{15}$; 14 days.	Discission, 1. 16. 96.	$\frac{6}{6}$	One strand across pupil; fundus easily seen and normal.
10	M.	65	Chronic bronchitis.	Immature senile. V. = counting fingers at ½ meter	1. 28. 96.	Combined extraction; some cortex remaining in coloboma.	Patient very restless; anterior chamber remained open 5 days.	$\frac{6}{20}$; 14 days. $\frac{6}{12}$; 6 weeks.	Discission, 3. 19. 96.	$\frac{6}{6}$	Clear pupil.
11	M.	40	Good.	Immature traumatic; v. = l. p.	4. 25. 94.	Small corneal flap, with iridectomy; considerable cortex remaining.	Uneventful.	Counting fingers at 1 foot; 10 days.	Declined.	$\frac{6}{12}$	Cortex gradually absorbed, leaving some thickened capsule blocking the coloboma, which contained several small openings; 10. 10. 94, V. = $\frac{6}{12}$? Patient seen one year later, and V. had declined to $\frac{6}{30}$, owing to thickened capsule; declined discission.
12	F.	40	Good.	Immature senile. V. = counting fingers; lens slightly swollen.	2. 12. 96.	Combined extraction; a large piece of cortex remaining.	Uneventful.	—	—	—	Patient under her physician's care, who reported gradual absorption of the cortex, and the pupil now quite clear, 3. 6. 96. Glasses not yet ordered; patient attending to household duties.

sion was performed, in two it was declined, in one it has not yet been performed, and in the remainder it has been unnecessary.

In two patients vision was normal, in three one-half of normal, in two two-fifths of normal, in one one-fifth of normal, and in two one-sixth of normal. In one vision is good (the patient has not yet been tried with glasses), and in one vision was counting fingers, the patient being able to move about. The poor vision was due to alcoholic atrophy of the optic nerve, which was present before the extraction of the cataract. The two cases with one-sixth of normal vision are the cases of high myopia before referred to, and, considering the extensive lesions of the fundus, this result is excellent. The case with one-third of normal vision, declined discission, which would certainly have raised the acuity of sight; of the cases with two-fifths ($\frac{6}{15}$) of normal, one had an old corneal macula, and the other has not yet submitted to a needed discission.

The ages of these patients varied from twenty-seven to seventy-five; four of them had reached that age in which, to quote the sentence in the beginning of this article, "the ordinary criteria of ripeness" need not be regarded and the lens may safely be extracted. The others were under this age.

The health of seven of these patients was good; in two it was feeble; one had albuminuria, one chronic bronchitis, and another was a chronic alcoholic with atrophy of his optic nerves.

I have not included in this list several other cases which properly belong here, chiefly for the reason that I do not know exactly what the final outcome was. One, for example, was a case of partially formed cataract, the probable result of an injury. The lens was extracted and the healing kind. The patient, however, left the hospital without permission, some cortex remaining in the coloboma, and never subsequently reported. There is no record of the vision other than that he could see to tell the time on the face of a watch. In another very interesting case, which I reserve for a separate publication, there was a degenerated lens, also the result of traumatism, and which I extracted in its entirety with capsule-forceps, but not without loss of vitreous, ultimate vision, however, being very good. I have not included in this list those instances with which we all are familiar, in which, although the lens comes out cleanly, it is yet an unripe lens; lenses, in other words, which never reach full maturity, or, at least, never reach the condition of perfect opacification.

If I were to sum up my belief in regard to the extraction of immature cataract, I would say with Knapp, with White, and I suppose with all surgeons, that I prefer always to wait for maturity, or for that time of life when the lens, even though immature in the ordinary sense of the term, will cleanly leave its capsule. But I prefer the extraction of immature cataract (combined section) to the performance of an operation for ripening. I perform preliminary iridectomy if functional examination determines that there are complicating circumstances, as, for example, in the cases of high myopia, and in two of my cases of immature nuclear cataract in which the tension of the eyeball was diminished and there was tremulousness of the iris, indicating probable vitreous changes.

DISCUSSION.

Dr. H. F. Hansell said that the question of extracting unripe cataracts is one that concerns every ophthalmic surgeon. Cases are constantly occurring in which one wants to know what procedure it is best to follow. There is a misconception in the minds of some medical men—who think a lens is hard only when the cataract is mature; and forget that a lens may be entirely opaque and yet soft and unfit for the operation of extraction. Dr. Hansell referred to a case of congenital cataract, in a patient twenty-seven years old, in whom both lenses were involved. They were extracted in their entirety. The congenital lamellar opacity was distinctly marked, while the cortex of each lens was clear, yet hard. At the time, Dr. Hansell wrote Dr. Knapp, asking at what age the lens is sufficiently hard to extract—when to discard the needle-operation and resort to extraction. He answered that the earliest age was fifteen years. Dr. Weeks says in his paper, that after twenty-five the lens may be extracted, even though the entire lens may not be included in the opacity. The Germans, who are operating extensively for the cure of high and progressive myopia, extract without regard to the age of the patient. They operate for all cases over ten D. Dr. Hansell's own experience is limited, but in one patient, aged forty-eight, who had lost an eye in an accident, he ripened the incipient cataract in the other eye by Forster's procedure. The wound healed kindly, and six weeks afterward he extracted a perfectly mature cataract. He believed the preliminary operation was unnecessary and that he could have extracted the lens just as well without dividing the operation into two parts. He thought it best to follow Dr. Weeks in this matter, especially in patients whose appearance and vitality do not correspond to their age.

Dr. de Schweinitz said that each surgeon necessarily rests his belief on the value of the ripening operation for cataract on his own experience. His was limited and thus far had been without accident, but because he had seen such serious consequences of ripening cataracts in the practices of other surgeons, he was inclined to look upon this operation with disfavor. He had seen the excellent results of Dr. Jackson's ripening operation, who employs the White-Pooley method. None the less, he believed that Weeks is right—namely that it is better to do one operation on an unripe cataract,—viz. extraction without preceding manipulations for the purpose of increasing the opacification of the lens.

CASES OF ARTIFICIAL ANUS AND FECAL FISTULA TREATED BY INTRAPERITONEAL OPERATION.*

JOHN B. ROBERTS, M.D.

[Read March 25, 1896.]

INTESTINAL RESECTION FOR THE CURE OF ARTIFICIAL ANUS MADE FOR THE RELIEF OF INTERNAL STRANGULATION FOLLOWING HERNIOTOMY FOR STRANGULATED HERNIA; RECOVERY.

A man aged sixty-seven years was admitted to the Methodist Hospital on November 21, 1895, with strangulated inguinal hernia of the right side. He had suffered with this hernia for about forty years, and it had previously been operated upon twice because of the occurence of strangulation. Upon examination it was found that there was a small hernia on the left side and a large inguino-scrotal hernia on the right side which was strangulated. I promptly cut down upon the hernia, relieved the strangulation and did an operation for the radical cure by the method of Bassini. The hernia consisted of the entire cecum, which showed a dimple-like depression at one point, evidently representing the appendix, which was congenitally absent. Subsequent inquiry of the surgeons who had operated upon the man previously proved that his appendix had never been removed, and that his case was therefore one of congenital absence of the organ. There were a good many old adhesions uniting the coils of intestine in the neighborhood of the cecum.

The patient showed very little shock after the operation, and did well for one day, when his abdomen became somewhat tympanitic and painful. The next day the pain and tympanites had increased, and vomiting occurred when attempts were made to have his bowels moved by sodium phosphate. I determined to make an incision in the median line, because it was evident that there was an obstruction in the intestinal canal.

Active peritonitis was present and a portion of the small intestine just above the ileo-cecal valve was found to be dark in color, as if it had just been relieved of some encircling constriction. It was evident that this portion of the intestine, about two inches long, had been strangulated by being entangled in some of the twisted masses of adherent intestine resulting from the previous attacks of inflammation. This bowel was so dark, and the activity of the peritoneal inflammation so great, that I made an incision into the gut and allowed a quantity of feces and gas to be expelled. Being afraid to put this dark and gangrenous-looking bowel back into the abdomen, I stitched the knuckle of intestine to the center of the incision. The opening through which I had permitted the evacuation of feces and gas was closed with a hemostatic forceps, and the wound above and below the protruding bowel closed with sutures. Before doing this I also punctured the intestines within the abdomen in another place with the aspirating needle, to allow the escape of gas, and closed the opening with sutures.

After the intestine had been stitched to the abdominal wall I found that it had regained its normal color. This

showed that it is probable that the gut would not have become gangrenous at this point and could have been returned, though it is questionable whether such a proceeding would have been wise when the distention and the violence of the peritoneal inflammation were so great.

Inflammation of the belly-wall around the protruding knuckle of bowel took place and an abscess formed in connection with some of the stitches. After the tympanites and active signs of intra-abdominal inflammation had subsided, I endeavored to close with sutures the opening which had been made in the protruding portion of the bowel. Two attempts were made to do this, but both of them failed, because gas and feces forced their way through the opening, notwithstanding the presence of the sutures applied after the method of Lembert. The final result, therefore, was the production of an artificial anus in the intestine left stitched in the incision.

The anterior surface of the abdomen became the seat of a very painful and extensive eczema which caused the patient much pain, and gave us a great deal of annoyance. On January 11, 1896, I operated for the cure of artificial anus, although I thought the operation was likely to be much complicated by the difficulty of preventing infection from the eczematous skin.

An incision was made above and below the opening in the bowel; this was then detached from the abdominal wall and the adhesions of the neighboring portions of intestine separated. During this stage of the operation my finger tore the bowel just above the artificial anus. Four inches of gut which included this tear and the abnormal anus were then resected and a circular suture of the intestine made with two rows of Lembert sutures of silk. No portion of the mesentery was removed, but the portion corresponding to the removed bowel was folded. A drainage-tube was introduced because of the probability of infection, and the wound closed. The subsequent history of the case was unimportant, though there was some suppuration about the stitches, one or two of which were subjected to great tension because of the retraction of the belly-wall at the seat of the artificial anus. The patient recovered without special symptoms worthy of record.

TWO CASES OF FECAL FISTULA, RESULTING FROM SLOUGHING HERNIA, CURED BY CELIOTOMY AND INTESTINAL SUTURE.

A woman, over sixty years of age, had been operated upon several years previously for strangulated femoral hernia on the right side. On examination at the Woman's Hospital I found a minute orifice in the right groin, from which a small amount of liquid feces escaped more or less continuously. The tissues around were irritated by the escaping fluid. The opening would only admit the end of a probe. I therefore concluded to attempt closure by making an elliptical incision around it, dissecting out the tissue at the edges and suturing the wound left by this procedure. After making the elliptical incision, the wound and fascia were dissected up, the mucous membrane lining the small opening in the intestine turned inwards and catgut sutures applied in such a way as to close the intestinal orifice. The edges of the integument were brought together by interrupted sutures of silk, and an occlusive dressing of iodoform and collodion was then applied.

The operation, however, was unsuccessful, and fecal matter soon began to ooze through the wound, which failed to unite. The patient became very much discouraged and had rather serious symptoms a couple of weeks after the operation from partial suppression of urine. This condition, however, had no relation to the operation. After getting her kidneys to act more satisfactorily, I made, on November 15, 1895, four weeks after the operation just described, a four-inch incision in the median line of the abdomen, between the pubes and the umbilicus, and elevated the pelvis so as to throw the intestines upwards toward the diaphragm. A loop of small intestines was found adherent to the edges of the internal femoral ring. The external opening, through which feces had been escaping, communicated with the interior of this loop of bowel. A piece of gauze was carried

beneath the adherent coil, and the rest of the intestines protected as much as possible by pads of gauze. I carefully separated the bowel from the abdominal wall around the margin of the ring, and drew it out of the incision. A double row of Lembert's sutures of silk were used to close the opening in the detached bowel. The line of suturing was made transverse to the long diameter of the gut in order to make as little constriction at that point as possible. The fistulous opening was then cut out by transfixing the abdominal wall, after thorough cleansing of the excoriated surface of skin around the site of the fistula. Catgut sutures were passed through the abdominal wall to close the opening, and the peritoneal surfaces beneath were drawn together by catgut sutures introduced from within the abdomen. The abdominal incision was then closed in the ordinary way with silk sutures. The convalescence was uninterrupted. The beneficial moral effect of the operation upon the patient was very marked.

I had first intended to operate by the intra-abdominal route, when the woman first came under my care, but was led to do the less serious operation by the exceedingly small orifice through which the contents of the bowel escaped.

A year and a half ago I operated upon a similar case in a feeble, old woman, of sixty-four, who had had for about two years an artificial anus in the left groin, due to a sloughing femoral hernia. An unsuccessful attempt to close this fistula had been made, it was said, at the Presbyterian Hospital, about fifteen months before I saw her at the Jewish Hospital. Fecal discharges occurred in small quantities almost constantly, and the woman seemed greatly depressed by their occurrence. Her age and feebleness made me hesitate for a long time to operate upon her. In October, 1894, however, I finally concluded that the risk was justifiable. The case was not one suitable for using the Dupuytren enterotome, as there seemed to be no special spur to be divided. A median incision was made and the patient put in the Trendelenburg position. The attachment of the bowel to the abdominal wall was much more extensive in this case than in the other. The fecal fistula was really sufficiently large to be called an artificial anus, for nearly all the contents of the bowel escaped here. This was quite different from the condition of the other patient, in whom nearly all the alvine evacuations passed through the normal route. In both instances an effort was made to prevent contamination of the operative wound by the discharges coming from the fistula. In the present case, I sewed the cutaneous margins of the fistula together over the orifice before making the median incision. The gut, where attached to the abdominal wall, was encircled by a ligature, which was drawn sufficiently tight to prevent the escape of feces into the abdomen when I detached the intestine from the neighborhood of the femoral ring. This was done because the opening was a large one, and I feared fecal material would pour out into the peritoneal cavity. After the bowel was cut loose with the scissors, intestinal sutures were applied, so as to close the opening in a transverse direction. The bowel was then dropped into the abdominal cavity. This step of the operation left an opening in the anterior abdominal wall at the site of the femoral canal lined with mucous membrane, which had been left by cutting away the attached intestine on the inner side. The woman's condition was rather bad, and I therefore did not dissect out all this tissue and bring the raw surfaces in contact as in the first case. Some of the mucous membrane, however, was dissected away. The rest was allowed to remain and the opening closed spontaneously by Nature's efforts. This portion of the operation I considered unsatisfactory, because of the prolongation of convalescence. It would have been better if I had encircled the opening with an incision through the entire abdominal wall and brought the parts together, so as to get primary union. This is easily done when the abdominal cavity is open, because the surgeon has access to both sides of the orifice. In the other case this method was used

satisfactorily. With a sponge pushed up against the internal orifice I was able to prevent wounding of the intestine when the knife was entered from the external surface, and also avoided contaminating the peritoneal cavity with secretions from the inflamed skin around the old artificial anus.

DISCUSSION.

Dr. E. W. Holmes contended, with regard to the time of operation, that this should be performed as soon as possible after the inflammatory symptoms have subsided and adhesion has taken place. After this there is considerable cutaneous irritation and eczema, which cause great inconvenience to the patient and may interfere very much with the nutrition. After the fistula is established, there may be constriction at the distal end of the bowel, which would interfere with the passage of feces and add to the difficulty of the operation. Dr. Roberts said very little with regard to the form of operation. It seems that it is a question whether or not the Murphy button should have been used, as it might have expedited the operation in this case. When the intestines are adherent to each other, it would perhaps be unwise to use the button, as in some cases it might not pass down easily. Since the success which attended Dr. Roberts' operation, Dr. Holmes has had a much more favorable opinion of circular enterorrhaphy than before. Dr. Roberts performed the operation by putting the sutures at four points equi-distant and then completing with a double row of Lembert sutures. In the case at the Methodist Hospital, the bowels were so matted together that it may have been impossible to separate them. The success of Dr. Roberts' operation has justified his decision in the matter, but it might be interesting to discuss this question of adhesion, as many obscure symptoms result from them. Examination of the viscera of many bodies has shown that adhesions are very common and patients with various forms of abdominal injuries frequently suffer from obscure symptoms which seem to be the result of adhesions. As to the necessity of breaking up such adhesions, reference was made to the statement by a prominent gynecologist recently that he never broke up intestinal adhesions, while another prominent gynecologist always breaks them up with greatest care, and this would seem perhaps the more rational method of treating these conditions, although it would prolong the operation.

Dr. Mordecai Price said that the question of artificial anus brings up several questions. It is nearly always the result of strangulated hernia, or the choice of the surgeon. It is produced by hernia when the bowel is completely strangulated. There is no question about the propriety of the operation, or the kind of operation. There is probably no method as good as the Murphy button and end-to-end union. There should be complete separation of the bowel from its adhesions and also all adhesions in the abdomen, for the simple reason that, if they are left in complete hernia and much time has elapsed between the time of the formation of the artificial anus and the time of operation, the surgeon has no proof of the patulous condition of the bowel. It is, therefore, better to release all adhesions and bands. It would be impossible to use the Murphy button when the bowel is thickened and indurated at the seat of operation. It has been Dr. Price's method to resect such portions as are thickened, and introduce the Murphy button into a healthy portion of the bowel. After being placed in position, this union is reinforced by a Lembert suture, and then the mesentery is sutured. Failure or leakage does not follow this use of the button. It is useless to do the operation without releasing adhesions. Dr. Price related an exceptional case in which serious trouble followed this procedure. An artificial anus was established within a short distance of the stomach. The patient was much emaciated and in bad condition. Many feet of bowel matted together in the region of the strangulated hernia had to be separated and quite a large piece removed. The patient was unable to stand the operation. Although she lived for several days, she never properly reacted. From Dr. Roberts' description of his case and from the excoriation of the abdomen from the discharges, it would seem that the opening in the bowel must have been high up. It has been Dr. Price's experience in these cases that the digestive fluids high up in the bowel excoriate very much. A number of surgeons have performed lumbar or inguinal colotomies in which the skin was almost unaffected.

In those cases in which the irritation is very great and the lesion in the bowel high up, the necessity is greater for immediate surgery. Dr. Price considered the Murphy button a life-saving instrument. He believed that the method is all right and that it applies perfectly and that the time of operation is greatly shortened, but he would hesitate to use the Murphy button in what is called anastomosis, when a knuckle of bowel is left out and when the button may drop in the loop. On the other hand, he would have no hesitation in uniting

the bowel with a simple suture and had done so a number of times. The old methods of anastomosis were all right in their way, and it was simply a step in the right direction. Dr. Price had used the Abbe rings in several cases with great satisfaction. With regard to fecal fistula, unless the bowel comes into direct contact with the abdominal walls, the fistula should always close. This has been demonstrated in many cases. Dr. Price has seen a number of cases in which the bowel was patulous and the gangrenous process had gone on in its destruction at the head of the colon in bad cases of appendicitis, leaving a number of holes at the head of the colon. Light packing and rubber drainage were followed by free discharge of fecal matter from the bowel. Union had been perfect in all these cases in the course of ten days or less. Fecal fistulæ may occur in the descending colon, in the sigmoid flexure and in the transverse colon as a result of inflammatory conditions. In other words, a gangrenous ovary and tube lying in contact with the bowel are likely to lead to gangrene of a portion of the bowel.

With regard to glass drainage-tubes, various accidents have been unjustly attributed to their use. Dr. Price referred to a case presenting a gangrenous spot in the bowel in a case in which he knew that a fecal fistula would be the result. To avert that condition he used glass drainage. At the end of forty-eight hours gas and contents of the bowel were being discharged through the tube.

Dr. E. E. Montgomery endorsed Dr. Roberts' method of opening the abdomen, as by that means the surgeon is better able to bring healthy ends of the bowel together and insure more thorough and certain union. The only criticism to be offered concerned the case in which the intestine was brought up and attached to the opening. It would perhaps be a better plan to have resected the intestine as a primary operation rather than to have left it as long as it was left, and allowed the continued suffering of the patient. With regard to the adhesions of the intestine, it seems better to break them up, as one is uncertain in regard to the patulousness of other portions of the bowel. The method of treatment is an important one. Ordinarily, breaking up the intestines leaves a raw surface which may become adherent and possibly give rise to disturbance subsequently. In a case in the Philadelphia Hospital some years ago, following an operation by one of Dr. Montgomery's colleagues, the patient had all the symptoms of obstruction. In this case a large amount of the bowel was broken up and a volvulus was found. When this twist was undone, the abdominal cavity was filled with a solution of boroglycerid and this was left in the abdomen. A drainage-tube was introduced and through it the same fluid was occasionally injected. Dr. Montgomery had an opportunity of operating upon this patient some two years later for ventral hernia. Remembering the previous operation, he expected to find many adhesions, but on opening the abdomen there was but one adhesion. This result shows how these cases will recover.

Dr. Roberts added, with regard to Murphy's button, that he had often had it with him in operating, but he had never yet used one because he had thus far always decided to use suturing, and possibly because he did not like to complicate things. In the case mentioned, in which he did a resection and end-to-end union, he thought very strongly of putting in a Murphy button, but did not because the old adhesions would have compelled the button to travel a very tortuous course and there was danger of its becoming entangled in the lumen of the intestines.

AN UNUSUAL ALVEOLAR ABSCESS, WITH ANTRAL COMPLICATIONS.

G. HUDSON MAKUEN, M.D.

[Read March 25, 1896.]

So far as I know, this case presents several unique features. The patient, a male, aged forty years, first consulted me with reference to a dull aching and somewhat indefinable pain in the gum covering the buccal surface of the first molar tooth of the upper jaw on the left side. The teeth were in fairly good condition, with the exception of some retraction of the gums in both upper and lower jaws. Further than this I could detect only a slight redness over the seat of pain.

I at once referred him to a dental surgeon of considerable repute, whose diagnosis was guarded and his treatment soothing and mildly counter-irritant. About a week elapsed, when to our surprise there appeared from under the gum along the tooth and between it and its fellow, the first molar, a slight discharge of thin pus. We then thought that we were dealing with a diseased pulp at the apex of the root, and the tooth was opened and the pulp found to be perfectly healthy. It appeared that we had an abscess entirely external to the tooth-substance and probably situated at or near the bifurcation of its roots. The discharge increased and we decided to drill through the crown of the tooth and try to drain and treat the abscess through this opening; but we failed to find the pus-sac.

The dental surgeon seemed entirely at a loss to explain the cause or the exact seat of the trouble, and upon exploring with a delicate probe one day this passed into the antral cavity at the point where the external wall of the antrum joins the alveolar process. He assured me that no force was used, but the patient complained of very severe pain. From this time the inflammation rapidly extended into the antrum and within three days we had to deal with an acute inflammatory condition of the maxillary sinus and the left nasal cavity and post-nasal space. Fortunately the ostium maxillare was patent, and the discharge, which was of a muco-purulent character, and very profuse, oozed freely through the nose both anteriorly and posteriorly. During this time the antrum was syringed through the alveolar opening. I advised the immediate removal of the tooth, but my consultant, thinking that the cause of the trouble was external to the tooth and independent of it, persuaded me to wait a few days longer, to continue to favor drainage and to use the usual stimulating and antiseptic lotions. But on the second day thereafter the inflammation became so severe and the discharge from the nose increased so enormously that I insisted upon the removal of the tooth. This had to be done with great care, for the bone-substance entering into the formation of the alveolar process and the external wall of the antrum was very delicate and thin and the roots of the tooth were large.

The tooth was removed, however, without accident and there was found to be a small pus-sac in the peridentium of the inner margin of the anterior buccal root about midway between its apex and its base. The sac was so small that I was slow to believe at first that it could be the sole cause of the disturb-

ance; but the tooth itself was, as I have said, living and perfectly healthy in all its parts, with the exception of this small spot on its covering membrane, and after the most careful examination of the alveolar cavity we could find no disease whatsoever. In still further proof of the fact that the cause originated at this spot there was no further discharge from the cavity, and within three days the antral inflammation subsided and the patient was practically well.

To my mind this case presents at least two very interesting features. First, the origin of the trouble: A pus-sac forms in the covering membrane of a tooth of a perfectly healthy man at a point considerably removed from all external influences. What was the cause of this formation? How did it get there? Second, the rapid development of the antral complications as soon as communication was made between the antrum and the alveolar abscess; and notwithstanding the general inflammatory condition of the antrum and the copious discharge, there was no pain whatever, the only symptom being that referred to the nose and the opaque condition found with the electric light transmitted through the maxillary bones.

DISCUSSION.

Dr. B. A. Randall said that the point made as to suction is well taken and can be verified by frequent observations. He had known of instances in which an inflamed antrum that had been perfectly permeable in the evening to the electric light was completely closed by the next morning, apparently as the result of blood extravasating into the mucous membrane rather than into the cavity. The sniffling efforts of the patient, caused by the slight irritation, had a cupping effect upon the lining of the antrum, distinctly and uncomfortably recognizable at the time, and formed the only explanation that could be given for such a rapid and complete clouding in such a few hours.

Dr. G. G. Davis said that inasmuch as there is only one opening in the antrum, he could not see how it is possible for suction to take place; because for suction to exist, there must be a posterior, as well as an anterior opening, or a vacuum would be created. As to drainage, such an excessive amount would indicate an overflow. The first opening in this case was apparently a small one, made through the tooth itself. For the excessive amount of secretion which is said to have been present, this would hardly be a sufficient opening. One could not call it free drainage, and as evidence of this condition, as soon as free drainage was established, the discharge ceased.

Dr. Makuen, in closing the discussion, said that the outer wall of the alveolar cavity and of the antral cavity was very thin and the probe passed up into the antrum just within this wall, and the plate of bone was felt to yield a little under the pressure of the probe, or the nozzle of a small hard-rubber syringe. As soon as the antrum became involved, the discharge appeared in the nose and diminished through the opening into the mouth. Dr. Makuen had observed in nearly all his cases that drainage takes place through the ostium maxillare even when there is a large opening through the alveolar process. He suspected that nasal respiration may be a causal factor in this phenomenon. The interesting points of the case are that an abscess should form on the covering membrane of a tooth so far removed from any external influence and that it should cause so much disturbance.

A CASE OF ACUTE LOSS OF VISION FROM DISEASE OF THE ETHMOID AND SPHENOID CAVITIES.

HOWARD F. HANSELL, M.D.

[Read March 25, 1896.]

R. D., aged seventeen years, awoke on the morning of February 13, 1896 with severe headache, located mainly in the frontal region, and almost absolute loss of vision. He had gone to bed the night before in his usual health, free from any disturbance of sight. In the early part of January he had a well-marked but uncomplicated attack of measles, which ran the usual course. Two weeks later he had entirely recovered, and resumed his occupation as driver and stable-boy. One night, being very tired, he threw himself on the floor behind the stove in the kitchen, and slept there till morning. On February 19th, he was brought to the Polyclinic Hospital by Dr. H. M. Beatty, of Trenton, N. J. During the week that intervened between the onset of the attack and his visit to the hospital, his vision had grown worse and his mental powers had become slightly dulled, but he no longer suffered from the excruciating headache that had tormented him for four days. Upon examination the following ocular conditions were noted: The lids and conjunctivæ were normal, the corneæ and anterior chambers clear, and the latter of normal depth; the irides were moderately dilated, and absolutely unresponsive to light; the lenses were clear; the vitreous chambers were clouded by a great quantity of minute opacities (punctate hyalitis); the fundus of each eye was dimly seen, but its condition could be determined with moderate accuracy; the optic discs were pale; the arteries slightly contracted, and the veins normal in caliber; the edges of the disc were not obscured by other exudation than that in the vitreous; there was no optic neuritis or choking of the disc; each retina was edematous and toward the periphery a few grayish, curved lines, marking linear detachments of the retina from the choroid, were visible in the lower parts. Vision was reduced to the perception of light in the extreme temporal parts of the field. The boy was emotional, easily excited and of slow mentality. Upon admission his temperature was 99°, the respirations, 24; the pulse, 104, full and strong. There was no pain in the eyes or head. The urine was free from sugar and albumin; its specific gravity was 1022. The patient was bled from the arm, about seven ounces of blood being removed; elaterin, one-quarter grain, was administered, and he was given a hypodermic injection of pilocarpin, one-quarter grain, and a hot bath. On the following day the pulse had fallen to 56, and was weak and compressible and occasionally intermittent; the temperature and the respirations were normal; intellection was more active. In the absence of an assignable cause for the ocular and cerebral edema, the boy was referred to Dr. Walter J. Freeman for examination of the cavities of his head. Dr. Freeman's report is as follows: The view of the interior of the nose was completely obstructed anteriorly by immensely swollen turbinals, but posteriorly muco-pus in

considerable quantity was seen flowing from the choanæ.

Thorough cocainization produced contraction of the congested intra-nasal tissues, and the fossæ soon became filled with thick purulent secretion, which came from both the superior and the middle meatus, right and left, and from the upper back part of the left fossæ. Transillumination of the maxillary and frontal sinuses, together with the absence of other symptoms of any disease of these cavities, pointed clearly to a diagnosis of acute purulent inflammation of the anterior and posterior ethmoid cells on both sides and of the left sphenoid sinus. The ethmoid arteries, branches of the internal carotid, through the ophthalmic, and the return veins which empty into the cavernous sinus, bring the affected cells into most intimate connection with the circulation about the optic chiasm. Then, too, the inflammation of the sphenoid sinus could readily act injuriously directly upon the chiasm, which immediately overlies it, and from which it is separated by a very thin plate of bone. Atropin internally, together with appropriate local treatment of the intra-nasal congestion, quickly caused the inflammation to subside, and in about ten days the nasal condition was normal, and there was no return of the inflammation of the sinuses.

Dr. S. D. Risley met me in consultation and confirmed the ocular findings already recorded. Dr. Risley hesitated to attribute the blindness to the ethmoiditis and considered the possibility of a common cause. Dr. G. E. de Schweinitz also examined the case, and his report was in accordance with my own findings; he made a diagnosis of acute double retro-bulbar neuritis from ethmoditis. During the succeeding days the general condition improved, the nasal inflammation was reduced by the treatment carried out by Dr. Freeman, and the vitreous opacities cleared so that the details of the fundus could be examined accurately. The edema of the retina grew less and less; the linear detachments disappeared, but the vessels, both arteries and veins, became smaller and the nerves paler; the outlines of the disc were clear, showing no exudation about the nerve-head. Day by day the ethmoiditis improved, until pus could no longer be seen coming from the ethmoid cells; the discharge from the sphenoid was the last to disappear. The cure of the ethmoid and sphenoid disease was not followed by recovery of vision, although the fields have gradually enlarged and now include the unbroken periphery, in which light-projection has returned, and fingers can be counted on the temporal sides, but a large central scotoma, in which light cannot be seen, persists. The pupils have become responsive to light, but are abnormally dilated; the arteries of the nerve and retina are almost thread-like, and the veins but little larger; whitish points, some glistening and some dull, and punctate disturbances of the pigment-layer can be detected throughout the retina.

In consultation with Drs. Charles K. Mills and W. W. Keen it was decided that there were no indications for operation on the skull.

No symptoms other than those described, namely, pertaining to the eye and the nasal cavities, were elicited after careful scrutiny. We must depend, then, on the ocular and nasal examinations to determine the source of the blindness. Unfortunately our conception of the processes that have caused such sad havoc are largely speculative. We know from Dr. Freeman's report that a purulent inflammation of the ethmoid and sphenoid cells existed, but we can only conjecture the relation of this to the blindness. As will be seen from Kuhnt's monograph,[1] similar effects have been observed in the course of disease of the frontal and ethmoid sinuses. Alt reports a case[2] of optic neuritis caused by cauterization of the turbinated bones, which rapidly grew better when the nasal treatment was discontinued. In analyzing the various ocular findings we shall consider them in detail:

1st. Punctate hyalitis, or the collection in the vitreous body of minute dust-like opacities, moving in waves with

[1] Ueber die Enzündlichen Erkrankungen der Stirnhöhlen, etc., 1895.

[2] *American Journal of Ophthalmology*, September, 1895.

every movement of the ball, and obscuring, to a considerable extent, the ophthalmoscopic view of the fundus. There was no appreciable difference in the amount of these opacities in the two eyes. While their exact source and nature are unknown, it is probable that they are the products of an exudation from the choroid in the neighborhood anterior to the ora serrata, where the choroid is uncovered by the retinal tissue. Their presence is reasonably accounted for by the supposition that, owing to a sudden increase of tension of the choroidal vessels, either from contraction of the walls of the vessels, or from increase of their contents by stimulation of the sympathetic plexus on the aorta or cavernous sinus, the fluid contents of the blood exuded in part into the vitreous body. Apparently opposed to this hypothesis is the fact that the intraocular tension was not increased; indeed it was slightly below the normal. There was, however, no damming up of the excretion through Schlemm's canal in the periphery of the cornea or disturbance in the flow of lymph from the vitreous into the anterior chamber, and hence no cause for increased tension. The excretion through the cornea was probably on the other hand hastened, as evidenced by the slight reduction of intraocular tension.

2d. The retinal edema was universal and more pronounced near the periphery, where the fluid had collected in curved lines in sufficient amount to separate the retina from the choroid. The arteries were contracted and the veins of normal caliber. The discs were well outlined and not more blurred on the edges than the retina in other portions of the fundus. The retina had lost its transparency, was gray in color and seemingly saturated with serum. There were no pigment-alterations or hemorrhagic spots, nor did the foveal region differ in color from that of the adjoining parts. This fluid exudation had one of two sources, namely, from the vessels of the retina, or from those of the choroid, or indeed from both. I am inclined to believe that it was derived in great part from the former, the circulation in which was indirectly reduced by interference with that of the venæ vorticosæ, thus cutting off the anastomosis between the choroid and the retina at the foramen sclera. In addition, the current of blood in the central vein of the retina was probably impeded in its course by an obstruction or diminution of caliber, either in the ophthalmic vein or in the cavernous sinus. As opposed to this view of venous obstruction we must remember important symptoms that were conspicuously absent, namely, choking of the discs, tortuosity of the veins of the retina, and choroidal or retinal hemorrhages. These conditions would undoubtedly have been observed if thrombosis of the choroidal veins or of the ophthalmic vein had been present. We are therefore forced to exclude this possible explanation of ocular complications in ethmoidal disease in our case.

3d. *Loss of Vision.*—There was sudden and absolute loss of light-perception on the temporal half of each retina and preservation of light-projection on the extreme temporal side of each field, or partial functioning of the nasal half or section of the retina, an indistinctly marked bi-nasal hemianopsia. The line of division between the perceptive and non-perceptive sections was not clearly made out. It was determined that central vision was lost in both eyes. In the left, the line of demarcation corresponded to a curved line from above-inward to down-outwards, with its concavity toward the nose. In the right, light-perception was retained only on the extreme nasal portion of the retina, and lost in the macular and all other portions. If we accept the hypothesis of a single lesion we must exclude purely local disturbances, such as thrombosis, embolism or orbital inflammation and refer the active agent back to the chiasm. This solution is the only tenable one. There was undoubtedly inhibition of function of the optic tract, complete for those fibers supplying the nasal half of the retina and partial for those running to the temporal—probably from pressure of the edema on, and edematous infiltration of, the chiasm and nerve-trunks. Direct and active pressure of a new and unyielding growth would have induced serous infiltration

of the sheaths and choking of the discs, rather than limitation of the circulation by encroachment on all the vessels by fluid exudation. This hypothesis is again opposed by the diminution in size of the veins of the nerve and retina. We should naturally expect, whatever may be the cause of venous obstruction, whether direct pressure at one point or along the course of the veins, dilatation, tortuosity and rupture of the retinal or choroidal veins—an obstacle that requires considerable speculative ability to overcome, as it seems to invalidate any explanation that has been offered. Kuhnt suggests that acute loss of vision in these cases is to be attributed to thrombosis of the central vein. When the blindness is bilateral the thrombosis must be double or must be situated in some intracranial sinus.

In considering the pathology we must remember that the symptoms common to ocular involvement in disease of the nasal cavities were in large part absent. For instance, there was no edema of the lids, no chemosis of conjunctivæ, no injection of the balls, no exophthalmos, no limitation of movement, no inflammation of the orbital tissues, no hemorrhages. The only symptoms presented by our case were pain in the head, not localized, edema of the vitreous and retina, contraction of the retinal vessels, imperfectly marked bi-nasal hemianopsia, and unresponsive dilated pupils and mental dulness. Later, there were absorption of the vitreous opacities; disappearance of the retinal edema; numerous scattered patches of retinal degeneration; restoration of light-perception and light-projection to all parts of the retina, with the exception of that supplied by the macular fibers; central scotoma; return of pupillary action; and recovery of former mentality. The heart and lungs were normal and there were no signs of kidney-disease (report of Dr. Mays), and no elevation, but a slight depression of temperature.

The most reasonable and least unsatisfactory explanation of the ocular complication is that of a localized meningitis, attended with marked swellings and edema of the periosteum covering the body of the sphenoid bone, induced by purulent disease of its cells, through contiguity of tissue. The optic chiasm was directly attacked by the swelling of its supporting structure. The cavernous sinuses were encroached upon, and the function of the sympathetic plexuses of these sinuses was altered, probably stimulated, with resulting contraction of the coats of the neighboring blood-vessels, through the action of branches given off by the carotid and cavernous plexuses to the ophthalmic, and, perhaps, to the Gasserian ganglia.

In the January number of the English edition of the *Annales d' Oculistique* appears an article by Pergens on "Amblyopia and Amaurosis after Hematemesis." The symptoms and ophthalmoscopic signs in some of the cases recorded closely resemble those of my case. The blindness, edema and optic-nerve atrophy are explained by post-mortem findings of endarteritis and spasmodic contraction of the blood-vessels of the optic nerve, retina and choroid.

In Kuhnt's monograph will be found references to the principal publications on the relation of disease of the nasal cavities to diseases of the eye. This writer bases the connection on the anatomic relations of the veins of the nasal cavities to those of the orbit. The orbit is surrounded on three sides by cavities lined with mucous membrane, the veins from which, namely, the supra-orbital, frontal, ethmoid, ophthalmo-facial, empty directly into the ophthalmic vein. Therefore, every considerable swelling and hyperemia of the whole or part of the mucous membrane causes a stasis or hyperemia at the origin of the ophthalmic vein, with consequences modified by individual peculiarities. He considers the absorption of chemically decomposed pus into the system as a chief factor in inducing all the functional disturbances; in other words, a common cause for the suspension of function of the muscular, refractive and nervous apparatus of the eye.

It is to be regretted that this communication is limited to a recital of the clinical features of the case and to a speculative consideration of the pathologic changes that have produced the disastrous effects on vision. The acute

atrophy of both optic nerves is probably a permanent one. Notwithstanding the efficient nasal treatment, improvement of vision has been disproportionately slight. Day by day the arteries and veins of the retinæ have become more and more contracted until they can scarcely be recognized as blood-vessels; the discs have become paler and more atrophic and the retinæ degenerated, until normal structure has almost disappeared; yet the case is one of deep interest because of its rarity and because it illustrates the intimate and fatal causal connection between nasal and ocular disease.

DISCUSSION.

Dr. B. A. Randall said that he had seen several cases something like that reported, but they are certainly unusual and raise a very difficult problem. The sinus-inflammation theory is the best to account for some of the symptoms; and the hemianopsia could possibly be explained by the retinal edema.

Dr. Hansell said that, up to the present time, the boy had regained light-perception in the temporal parts of the retinæ, but the central fields, in which vision was lost from the beginning, were still blind. He agreed that the prognosis was unfavorable.

A STUDY OF THE MAXILLARY BONES AND THEIR RELATIONS WITH SOME OF THE ADJACENT TISSUES.

M. H. CRYER, M.D., D.D.S.

[Read March 25, 1896.]

It gives me much pleasure to have the opportunity of showing you photographic slides made from sections of the maxillary bones and some of their associated tissues. In doing so I cannot but think that many of the points that were new to me when brought out by these sections are known to many of you; if this be true I hope that you will still find novel material illustrated.

These studies have been made after the same method which the otologist has followed in his specialty of the temporal bone, in connection with its surroundings. The rhinologist in the same manner has taken up the nasal chamber, with its adjacent parts. These specialists have carried on their researches by making sections in all directions through the particular region in which each was most interested, their sections being made not only from articulated skulls and cleaned bones, but also those with their tissues dried upon them. So, here in the same manner, the mouth has been taken up, with its surrounding tissues and relations. These special studies naturally overlap each other. Both the rhinologist and the otologist have long since discovered kindred interests in the same region and a more exhaustive study of stomatology will carry the student to, if not into, the fields of other specialists. The main points to be considered are as follows:

In the upper jaw.—

1. The maxillary sinus and its relations with the surrounding tissues from fetal life to old age.

2. The shape and size of the sinus at various stages of development.

3. The relations of nerves and vessels passing through and along its walls.

4. The opening of the sinus into the nasal chamber.

5. The lateral surfaces of the nasal cavities, showing the complex structures that are associated with the nasal portion of the superior maxilla.

In the lower jaw the principal facts to be observed are:—

1. The U-shaped character of the cortical portion and the cancellated tissue within.

2. The inferior dental canal or the cribriform tube of the inferior maxilla, and the small tubes running therefrom to the teeth and to their interspaces.

3. The relation of the roots of the teeth to the canal and surrounding tissues, and the manner in which the nerves and vessels reach the teeth.

4. The sliding forward of the cancellated tissue within the U-shaped cortical portion of bone.

Slide No. 1 represents an anterolateral view of the skull, giving a general idea of the various external bones of one side of the face and head. The articulation of the teeth is almost normal. The slide shows the relations of the bones of which we are to speak to the other external ones. The outer surface of the lateral and interior wall of the antrum is shown, the teeth having been denuded of the external plate of the alveolar process; and, so removed, it will be seen that the maxillary sinus has

been opened immediately over the roots of the first, second, and third molars, showing how thin, in this case, the bone is between the roots of the teeth and the floor of the sinus. It is also very thin over the points of the roots of the cuspid and first and second bicuspids.

I wish to draw attention to the position of the infraorbital foramen, which is generally described as the termination of the infraorbital canal, which passes through the solid portion of the infraorbital ridge to the groove in the floor of the orbit. Several of the slides to be exhibited will show that this canal often passes diagonally through the sinus, somewhat in the shape of a tube—a point I have not noticed described in any work on anatomy.

On the inner side of the orbit is the os planum of the ethmoid bone, and at the inner anterior angle will be seen the opening of the nasal duct; these structures will be again referred to when viewing the internal structures of the nose. We shall omit speaking of the lower jaw until further on.

Slide No. 2 shows a perpendicular transverse bilateral section, made by passing through the anterior portion of the orbit, maxillary sinus, and the first molar of each jaw on either side. The eye has been divided just in front of the crystalline lens. Between the upper portion of the nasal cavities are seen portions of the middle ethmoid cells. In about the center of the floor of the orbit and roof of the sinus, which is very thin in this case, we find the infraorbital nerve in its canal, as generally described, and, below, the nearly triangular cavity of the maxillary sinus is seen, with a partial septum crossing transversely from the inner to the outer wall.

In the angle of the right sinus can be seen the anterior buccal root of the second molar, while on the inner wall is a portion of the palatal root of the first molar. The buccal root of the left first molar is easily seen passing well up in the outer wall of the sinus.

The central portion of this and the following slide gives a fine view of a transverse section of the nasal chamber. The septum in the center is remarkably straight—very seldom found so straight. Above the septum is the crista galli, to which the falx cerebri is attached anteriorly. On either side of the septum, at its upper attachment, is the roof of the nasal chamber. Running down on either side a little distance from this are the middle turbinated bones, the scroll-shaped bones below, hanging from the outer walls, being the inferior turbinated bones. The superior turbinated bones cannot be seen in a section in this position, as they are situated farther back. The space from the under surface of the inferior turbinated bone and the floor of the nasal cavity forms the inferior meatus, into which the lachrymal duct empties the fluid from the eyes. The space between the middle and the inferior turbinated bones is known as the middle meatus, through which the frontal and maxillary sinuses have their openings into the nasal chamber. The fluids from the middle and anterior ethmoid cells also pass through this meatus. Between the walls of the ethmoid sinuses and the middle turbinated bones is the unciform process. The spaces between this process and the outer walls of the nasal cavities are the semi-circular grooves generally known as the hiatus semilunaris. It is in this groove-like place that the excess of fluids passes from the various air-cavities, including the frontal and maxillary sinuses, out of the antrum at its upper anterior corner. Sometimes the opening is in the roof of the sinus, as will be shown. This groove passes backward, the uncinate process forming a guard to the opening of the maxillary sinus and thus preventing any foreign substance passing into the sinus.

We often speak of the fluid passing out of the sinus. It is a question in my mind if such is the case in a normal condition. It is more than likely the law of supply and demand is so governed that the parts are kept moist only, and the opening at the top so arranged as to prevent the undue loss of the fluid.

The openings of the maxillary sinuses are at the top, thus preventing drainage while the subject is standing or lying on the back; while the openings of the other air-cells or sinuses are so arranged

as to make almost complete drainage. Sometimes we hear of a "sound" being passed into the maxillary sinus. This cannot be done in a living normal subject.

Slide No. 3 represents the anterior portion of the same section. At the upper superior angle is seen a string marking the passage from the antrum into the hiatus semilunaris. In the floor of the antrum will be seen another septum. On the left side of the subject will be observed the anterior buccal and the palatal roots of the first molars. In the outer and inner walls of the antrum, from a dental standpoint, the position of these roots, as shown in this and the last slide—also in the following one—should be very interesting. Abscesses of these roots could very easily open into the sinus. The extraction of these teeth, if not carefully done, would carry away part of the floor of the sinus. Also, by using much force in placing artificial crowns, the floor could be easily fractured. In the case of a breakage in extracting, the roots could easily be forced into the sinus by the use of root-forceps.

In the great majority of skulls examined belonging to the white race, roots of the molar teeth either pass up in the walls of the antrum or point directly to the floor, being covered only by a thin, conical portion of bone; and occasionally this lamina of bone is lost, leaving the point of the root uncovered. A specimen will be shown in which an abscess has broken through this thin partition into the sinus.

In the colored race the walls and the floor are much thicker; therefore, we do not, as a rule, find the roots of the teeth passing up into the wall or even near the floor of the sinus. The relations of the teeth with the sinus vary so much that no set rule can be accepted; in fact, all the bones and structures of the face vary greatly in themselves and their relations with other structures.

Slide No. 4 shows a section made perpendicularly, transverse just within the infraorbital ridge, by taking off the anterior wall of the maxillary sinus. In the upper portion is the anterior wall of the frontal sinus, on the left side the nasal septum, and within the septum the nasal cavity.

The inferior turbinated bone is seen passing from the external wall of the chamber between the upper portion of the turbinated bone and the nasal wall of the antrum. Here also is seen the anterior half of the nasal duct, commencing at the inner angle of the orbit and terminating in the inferior meatus. The hard palate and alveolar process also are seen. They are both about the same thickness in this specimen. This would not be a good subject for implantation of teeth. The right picture is a section made near the posterior wall of the antrum and the orbit, from the same subject as the other. The walls of the antrum, it will be noticed, are very thin, and on the upper right corner will be seen the posterior ethmoidal cells. These exceedingly thin bones generally indicate old age.

Slide No. 5 is a section immediately between the the two just passed, showing its anterior surface. On the upper right corner is the frontal sinus. These sinuses vary much in size and shape; the left may pass far over to the right, or *vice versa*.

To the left is the orbit, with part of the eyebrows showing. In the center of the wall between the orbit and the sinus will be seen the infraorbital canal, below which is the maxillary sinus, which in this case is very large. In the upper portion in the nasal chamber is seen the middle turbinated bone, below which there is a string passing through the middle meatus into the opening of the maxillary sinus. The posterior half of the nasal duct is shown, passing from the inner angle of the orbit to the inferior meatus.

The hard palate and the alveolar process are again shown to be about the same thickness.

Slide No. 6 shows two pictures, the left one being removed from in front of the right one. When speaking of the first slide, the general description of the infraorbital canal was given. This slide and a few others following will show that the description given will not answer for all. These sections were

made by removing the anterior wall of the antrum, passing perpendicularly downward. From the center of the floor of the orbit there is a septum, cutting off a portion of the sinus, forming an infraorbital sinus, which of course is continuous with the true sinus. In the center of this septum-like wall will be seen a tube-like canal with the infraorbital nerve in place. Above this and at the junction of the septum with the floor of the orbit is an adjunct infraorbital nerve and canal. At the right is the hiatus semilunaris, into which there are many openings, among them that to the maxillary sinus.

In the right-hand picture, to the left of the septum is the opening from the infraorbital sinus into the true maxillary sinus. In the upper right-hand corner of the sinus the opening into the nasal chamber is well shown, passing into the hiatus semilunaris, guarded by the unciform process.

Slide No. 7 is a sagittal section just external to the infraorbital foramen. In the left picture there is, passing through the foramen and the canal, a piece of paper showing its direction, with a clear space over the tube-like canal. Another point of interest in this picture is the elevation of the bone over the roots of the first and second molars. Also within the floor of the sinus will be seen an opening through which an abscess has broken into the antrum from the palatal root of the first molar.

There are also elevations in the specimen over the roots of the first and second bicuspids, not seen in the picture.

In the left upper corner of the right picture is the outer portion of the infraorbital sinus, the lower border of which is about on a level with the tube for the passage of the infraorbital nerve and vessels. Below this process is an opening through the external wall of the maxillary bone. In this case the maxillary sinus extends into the malar bone. The teeth shown are the outer sections of those seen in the left picture.

Slide No. 8 is somewhat similar to the last one, being a sagittal section made internal to the infraorbital foramen. Here, again, the tube-like canal passes below an infraorbital sinus, a division of the true antrum. In the left picture, on the outer wall of the sinus will be seen grooves, the principal one running somewhat horizontally forward, giving off branches to the roots of the teeth.

It is often described that the artery and nerve pass to the teeth in the substance of the bone, from the posterior dental foramen in the tuberosity of the bone, and branches are given off in the bony structure.

This is the case only when the alveolar process is very thick and the sinus very small, and the roots of the teeth are not merely covered by a conical portion of bone in the floor of the antrum. But it is found in nearly all cases as shown; the nerves and vessels pass into the posterior dental foramen, then go through the bone into the sinus, passing into a horizontal groove, small branches passing from that to the teeth, the vessels and nerves being covered by the muco-periosteum while within the antrum.

Sometimes these grooves pass into small canals for a short distance. The main groove passes forward and terminates in the anterior dental canals, the passageway for the nerves and vessels to the anterior teeth. These latter structures are continuations of the posterior dental nerves and vessels. Immediately below the septum of the sinus in the anterior wall are seen several small openings where the anterior dental canals have been cut across.

In the floor of the antrum will be seen the conical cap to the roots of the teeth. Posteriorly is an opening over the buccal root of the third molar; anteriorly is an opening over the root of the second bicuspid.

The large opening in the right picture is the ordinary one found leading into the nasal cavity, as seen in the dried unarticulated bone.

Slide No. 9 is a sagittal section made near the inner wall of the orbit. The frontal sinus is seen at the top, and below this is the inner wall of the orbit, os planum of the ethmoid, including the edge of the inner portion of the floor, below which is the internal wall of the maxillary sinus. At the bottom will be seen an elevation of a tooth-socket.

This specimen is made from an almost edentulous person.

It is my opinion that, as the teeth are lost, these elevations disappear and the floor of the antrum becomes smooth and comparatively even. The small divisions or septa, as shown in the second and third slides, disappear with age.

To locate the opening and the direction in which the excess of fluid would pass from the sinus, a wire has been passed backward into the hiatus semilunaris. A perpendicular probe passes through the upper portion of the opening of the sinus, which in this case is partly in its roof or at the angle of the internal wall and the roof; as the straight probe passes out of the antrum, it passes through the infundibulum into the frontal sinus, showing that fluids could pass directly downward from the frontal into the maxillary sinus. These are specimens showing where the anterior ethmoid cells open just at the maxillary sinus. If the hiatus semilunaris should be closed by pressure on the septum or inflammation on the mucous membrane, the fluids from the frontal and ethmoid cells would pass into the antrum.

I had a patient during the past winter and spring who was troubled in this way, and in whom developed a fistulous opening into the mouth. The sinus was treated for several months without success. The natural opening of the sinus was closed, as proved by forced injections into the fistulous opening in the sinus. The fluid would not pass into the nasal chamber, and finally a large opening was made in the outer wall of the sinus, in order to make a better examination with the finger.

There was found a small tumor just below the natural opening. A curved probe could be passed through the opening in the direction of the frontal sinus. The tumor was then removed and a good-sized opening made into the nasal cavity, the patient recovering in a very short time.

Slide No. 10 is from a similar section of a different subject. It shows the opening of the sinus, running upward, where a small portion of bone has been cut away.

Slide No. 11 is a horizontal section made just below the floor of the orbit, the upper picture showing the roof of the two sinuses, with the nasal chamber and its septum between them. It will be observed that the openings of the sinuses are in the roof; on the left side there is a small probe passed into the sinus; on the right side a portion of bone is cut away, to show the direction of the canal-like opening. In the lower picture a fair idea of the shape of the upper portion of the sinus is given. The two round openings seen on either side are for the passage of the lachrymal duct.

Slide 12 represents a section made from the same subject as the last one. It is a horizontal one, made through the middle of the orbit, and gives a good view of the anterior, middle and posterior ethmoid cells, and the septum of the nose at its upper portion.

Slide No. 13 is from a transverse section of a superior maxilla. The right picture is made a little above the floor of the sinus. It shows the elevations and depressions in the floor; in the center of the largest depression there is a small spicula of bone, an exostosis, caused by an abscess of the palatal root of the second molar; on the rim of bone just in front of the sinus is a canal for the passage of nerves and vessels to the anterior teeth. The center picture shows where a section has been made a little lower down, through many of the roots of the teeth, although leaving portions of the floor of the sinus in two places. The point of the palatal root of the first molar is in view, with a large opening on its posterior aspect, caused by the abscess spoken of. The left picture shows the cap turned over from the middle one.

Slide No. 14 is made from the skull of a seven-months' embryo. It is a transverse perpendicular section, just within the floor of the orbit. In the upper portion will be seen two openings into the brain-case, with the crista galli and the falx cerebri between them; below this is the nasal chamber, with its septum. Hanging from the outer walls of the chamber are the middle and inferior turbinated bones. In the middle meatus there is seen the unciform pro-

cess, passing upward and a little inward from the base of the inferior turbinated bones. To the outer side of this process is the hiatus semilunaris, from which there is a small opening passing into the maxillary sinus, which is very small at this period of embryonic life.

The development of the sinus commences about the fourth month of fetal life by an invagination of the lining membrane of the nose within the hiatus semilunaris, in the position as shown in the picture. At the time the invagination commences and the eruption of the permanent teeth, the greater portion of the superior maxilla is occupied by dental organs. As the invagination takes place and progresses, the cancellated portion of the bone becomes absorbed. This absorption of the internal portion of the superior maxilla is carried on in various degrees throughout life, until in old age the walls become exceedingly thin, as shown in slide No. 4. As this absorption takes place, the roots of the bicuspids and molars are encroached upon, until in many cases the points of the roots are covered only by a very thin lamina of bone; even this is sometimes lost, leaving only the muco-periosteum. The re-absorption and enlargement of the sinus occasionally extend into the malar bone, as shown in slide No 7. At first the sinus is somewhat spheroidal, but it eventually takes a more triangular shape.

The dividing lines between the malar and the maxillary bones are plainly seen. In the lower jaw it will be observed how much space is occupied by the germs of the teeth, leaving very little space below for nerves and vessels.

Slide No. 15 represents three permanent teeth, a central, lateral, and cuspid, that were removed from a living subject about eight years ago. The patient was past middle life and suffering from neuralgia. He had no teeth in the process of the right upper maxillary bone, the region of his pain. Most of them had been extracted in the hope of giving relief.

The antrum was opened by Dr. Garretson and myself in search of the cause. We were somewhat surprised to see the three crowns protruding into the sinus; the roots were imbedded in the inner angle of the wall of the sinus. After being extracted by a small tooth-forceps the parts were treated in the usual way, and relief, with subsequent cure, was secured to the patient.

The crowns, it will be seen, are normal in shape and quite healthy, the roots being more or less defective. The pulps were alive, and it might be said that the nerves were being infringed upon at the points of the roots, thus causing the pain. I have seen several cases in which a greater portion of a tooth was found within the antrum, but I think this is the only case in which three have been reported.

Slide No. 16 represents two cuspids impacted in the upper jaw, lying at nearly right angles to each other. They were entirely covered by bone, and were exposed by a surgical bur. The end of the root of the right one is somewhat curved. There is but a slight layer of bone between it and the floor of the sinus, it being perforated by three small openings.

The roots of each tooth lie across the alveolar process, which has prevented the second bicuspids from being fully developed. The end of the right lateral incisor has been pushed out of the process altogether.

Slide No. 17 is made from the same jaw as the preceding one; it gives a view of the right and left sides. It will be observed that the roots of the first molars on either side differ very much from each other. The end of the root of the right lateral shown in the right picture has been forced out of its true position by the impacted cuspid of that side.

Slide No. 18 is a lateral view of the nasal chamber, showing the three turbinated bones and the three meati in the central portion. The frontal sinus is large and well shown. At the right hand upper corner the sphenoid sinus is shown.

Slide No. 19 is made from the same specimen as slide No. 18, having a greater portion of the inferior and part of the superior turbinated bones removed and turned upward, and showing the inside of a lid and the cells below. A section of the inferior turbinated bone

is removed, showing a probe passed into the lachrymal duct. The frontal sinuses are well shown, the upper one being the left sinus as it passes over from the left side; the under one is the right sinus passing inward and upward beyond the left, there being a very thin lamina of bone between them.

The infundibulum is plainly seen passing to the hiatus semilunaris. The hook-like process seen just below the infundibulum is the unciform process. In the recess back of this is the passage to the maxillary sinus, the process acting as a guard to the opening, and preventing a sound from being passed into the sinus in normal skulls. The anterior, middle and posterior ethmoidal cells, and the sphenoidal sinus, are well shown. In the posterior part of the nasal chamber the opening to the Eustachian tube is seen.

Slide No. 20. Usually there are three scrolls or turbinated bones, but this specimen shows three well-marked ones and two smaller ones, making five meati and five turbinated bones. In many animals, as the dog and hog, the upper portion of the nose, or that which presides over olfaction, is filled with a large number of scrolls.

Slide No. 1 is the first shown for illustrating the upper jaw. It will now be used to describe the lower one. The lower jaw is made up of a U-shaped cortical or dense bony structure, the ends of the U terminating in the plates of the alveolar process. It may be said to open at the top, and in normal conditions the teeth are inserted into this U-shaped space. A slightly compact tissue, somewhat cribriform in character, surrounds each tooth. The remainder of the space is filled with fine trabeculæ, forming the cancellated tissue, through which passes the inferior dental canal, which I prefer to call the "cribriform tube of the inferior maxilla." It passes from the inferior dental foramen forward on the inner side of the ramus of the jaw, downward and forward through this U-shaped space to near the incisors. At first it passes along or near the inner cortical portion of bone; when it gets near the mental foramen it crosses over nearly to the outer cortical wall. Its depth after leaving the ramus is in the lower portion of the U-shaped space, and passes beneath the level of the mental foramen. This tube can be isolated or removed from a normal specimen, as shown in slide No. 22.

As the tube passes along it becomes more and more cribriform. Beneath the first molar it becomes so opened that the tube-character is almost lost, but passing forward it again resumes its original character.

From this main cribriform tube there are lesser branches passing to convey vessels and nerves to the substance of the bone; also in more or less curved course to the roots of each tooth. A little anterior to the mental foramen there is a cribriform tube passing backward from the main tube to the foramen for the accommodation of the mental nerves and vessels. This return canal is almost invariably the normal arrangement, but occasionally this tube passes from the main tube as it approaches the foramen. It will be seen in the picture that the anterior portion of the wall of the mental foramen has been cut away, thus showing the direction of the returned tube.

The small lateral tubes which serve as nerve-conduits from the main tube to the roots of the teeth pass upward and forward in a more or less curved direction, varying according to the position of the tooth, those going to the third molar passing almost vertically upward; in those to the second molar the forward direction is greater; in those to the first molar it is increased still more; the one to the second bicuspid has the longest curve of all. Sometimes, instead of commencing at the main tube, this one is an offshoot of the tube to the anterior root of the first molar. The tubes going to the before-mentioned teeth all curve forward and upward from the main tube posterior to the mental foramen. The small tubes going to the roots of the first bicuspid and cuspid are branches from the recurrent tube to the mental foramen, and are curved slightly backward as they pass to these teeth. The tubes going to the two incisors curve slightly forward as they pass upward to the roots. The finer tubes going to the in-

terspaces and general cancellated tissue have the same general direction and curvature as those going to the roots of the teeth in their immediate vicinity.

The cortical U-shaped portion of the bone is the framework of the jaw, and forms the supporting structure; but it increases in length and breadth in a different manner from its contents. It is more than likely that it grows by interstitial process, each half having three fixed points, viz.: the ramus, the mental foramen, and the symphysis menti. There is no doubt that the distances between these points increase. The contents of this U-shaped space slide forward as the cortical structure increases in length, the teeth posterior to the mental foramen being pushed forward by each successively erupting molar, thus giving the curvature to the various small tubes going to the roots of the teeth, etc., and accounting for the stretching of the main tube until its character is almost lost under the first molar. This also explains the recurrent feature of the tube in relation to the mental foramen. As its end is attached to the walls of the foramen, the mass of cancellated tissue is pushed forward in its process of growth, and the tube itself is carried with it, making a loop. The reason that the small tubes going to the first bicuspid and cuspid curve backward is that their points of origin have been carried forward with the return tube, as they belong to that portion of the tube. The small tubes going to the incisors curve slightly forward as they arrive from the continuation of the main tube near the point where it curves backward to the foramen.

Thus we see that the anatomic structure, the relations of the various teeth considered in connection with the order of their development, and more especially the direction which the lateral branches of the main cribriform tube take in connection with the roots of the several teeth, furnish us with a permanent record of the method of growth of the inferior maxilla during the period between childhood and adult life.

Slide No. 21 is the left side of the lower jaw of slide No. 1. It is enlarged, showing the fine net-work of the cribriform tubes throughout the cancellated tissue; in fact, they form the greater portion of it, the special ones going in a slightly curved direction to the roots of the teeth. A wire in the mental foramen gives the direction of the return-tube or canal.

Slide No. 22 is taken from a specimen in which the cortical and the cancellated tissues have been cut away, leaving the cribriform tube isolated, showing it as an independent structure, and not merely a canal through the bone.

Slide No. 2, repeated. The right side gives a good idea of the U-shaped cortical portion of the bone heretofore spoken of, with the root passing into it. Below the root is the cribriform tube and the nerves situated within it. This gives an idea of the position of the nerves below the root of the first molar. It can be imagined what damage would be caused by a discharge of infectious matter from this root within this cancellated tissue.

The slide also shows the floor of the mouth and tongue cut transversely.

Slide No. 23 shows the anterior portion of the lower teeth in position. The principal object in showing this slide is to explain why it is often difficult to extract the second bicuspid from a jaw that has lost no teeth. It will be seen in this case that in a transverse section the roots of the second bicuspid are nearly in a line transverse with the anterior root of the first molar. The roots are also very long, extending below the root of the molar and nearly down to the inferior dental nerve. The bone at this point is usually more compact than farther back. Taking these points into consideration, this accounts for the difficulty in extracting these teeth without breaking them, and of course we must also consider that the root itself is not a strong one.

Slide No. 24 represents the lower jaw cut lengthwise nearly through its center, exposing the cancellated tissue, the sockets of the teeth, and the cribriform tube or inferior dental canal, with its branches going to the sockets of the teeth. As the tissue is very frail, much of the trabeculæ is lost. The outer section shows the direction of the recur-

rent tube for the accommodation of the mental nerve and vessels.

Slide No. 25 represents a metal cast, showing the size of the cancellated structure within the U-shaped portion of the body of the bone. It was made in the following way: After the openings of the sockets of the teeth were covered by pieces of paper, a slender tube about eighteen inches long was attached so that its lower opening was in connection with the inferior dental foramen. The bone and the lower end of the tube were invested in plaster of Paris with a a little asbestos. After this was thoroughly set and dried, and made about the temperature of 212°, fusible metal was poured into the tube, passing along the metal tube into the canal or cribriform tube and along its course, sending the metal out through its many openings into the cancellated tissue and the sockets of the removed teeth. The body of the bone and the lower portion of the ramus were placed in a ten per cent. solution of potassium hydroxid. This dissolved the bone away, except particles seen as white spots which penetrate the metal through the cancellated tissue. A transverse section of this preparation would show fine bony tissue passing all the way through.

The upper picture represents the inner surface, showing the size of the canal or tube, and it might be said that it represents the space occupied by the nerves, blood-vessels, and their membranes. The lower picture shows the outer surface, and the projection seen is where the nerves and vessels pass out of the mental foramen.

Slide No. 26 is a horizontal section of the upper and lower jaws. It is made a little above the free margin of the alveolar process of the upper jaw and a little below that of the lower. It gives the shape and position of the various roots, with their relation to the process and to each other at this joint. Particular attention is drawn to the fact that there is so little space between the roots and the plates of the alveolar process that it would be impossible to force the beaks of forceps between the roots and process without breaking the latter on one or both sides. The lines in the cut represent the strongest axes of the teeth.

Slide No. 27 represents a section cut horizontally through the lower jaw, made near the end of the roots, from the same subject as the last slide. The cancellated portion, with the soft tissue filling the spaces, is well shown. At the posterior part of the picture the nerve is seen passing into its tube. The ends of the roots of the second and third molars, the tip of one of the roots of the first molar, the roots of the first and second bicuspids are plainly seen. A little of the lateral incisors will be noticed. The centrals do not go down so far.

The illustrations we have been going over represent the lower jaw in the normal state. Those following will show various conditions of abnormality.

Slide No. 28 represents several sections of a lower jaw, not quite normal, there being evidences of past inflammation. Several teeth have been extracted before death, which changes the character of the jaw. Some of the sections show but one canal, while in other places there are many, and it takes close observation to tell in which the nerve and vessels have passed. In the operation of making a section or removing the whole nerve from the bone, a surgeon not familiar with this condition of things might easily clean out a portion of a canal, but not touch the nerve. This accident might not happen in a dried bone, but in the living the parts are so vascular, mistakes could be easily made.

At section D it will be seen that the distal root of the second molar penetrates the true nerve-canal. In the case of an abscess of this root, the discharge would flow into the nerve-canal; from this it could pass backward or forward along the nerve, and would give great pain by compression.

A baffling case of abscess on the external part of the lower jaw, just above the facial notch, can be explained, I think, by this specimen. The patient suffered great pain in the lower jaw. Finally a swelling made its appearance; the first molar was much diseased and was extracted, but this gave no relief.

The swelling was lanced, and a small quantity of pus was discharged, but this kept up for two months, when the man came to the hospital and was treated in various ways without success.

The teeth for a time were dismissed as being the cause, and cultivations were made from the discharge, to see if it were a tuberculous disease, but without results. The discharge was then examined by the microscope, to see if there were salivary corpuscles in it, but none was found. The second molar was in position and responded to heat and cold; finally a rubber-dam was placed over the second molar, and gently approaching the pulp-chamber, it was ascertained that the nerve in the distal root was in a decomposed state, while the anterior branch was vital.

This was devitalized at once and the decayed matter removed from the distal root, after which carbolic acid was forced down into and through the root by the use of softened gutta-percha.

In a short time the carbolic acid made its appearance at the external opening on the jaw. The tooth was filled with gutta-percha and the patient dismissed. From 4 P. M. until the next morning carbolic acid worked out through the opening, when the parts closed and did not again open. The question was then asked, how could a bone-fistula close so quickly, and why was there no discharge of broken-down tissue through the opening?

It can be explained by this section. If the discharge from the patient's tooth flows into the canal, like the one shown, it would have an easy passage along the canal, backward and out of the foramen, then down along the bone until it came in contact with the sheath of the internal pterygoid muscle, which could direct it into the submaxillary triangle and then pass out at its weak point, the opening for the facial artery. If such were the case, carbolic acid would clean this fistulous canal in such a manner that it would heal up at once.

Slide No. 29. Since making the preparation for the slide just passed, I have found a specimen in which the roots of a third molar passed out through the inner wall of the lower jaw a considerable distance below the mylo-hyoid ridge, as shown in the picture before you.

If this tooth had become diseased, it would have discharged its matter at once into the submaxillary triangle. I fully believe there are many unrecognized serious cases in which teeth of this character, when devitalized, cause pus to pass into the tissues of the neck, and even into the thoracic cavity. If diseased teeth in this region do not respond to treatment at once, they should be extracted. It is of much more importance than the saving of a tooth, as not only can ill health result, but death often occurs.

Slide No. 30 represents two views of an impacted third molar. In the upper one the tooth is in the position found when the cap of bone was removed; in the second view the tooth has been rolled out of its socket, showing its inner surface. The bed of the tooth is also shown. The second molar is a devitalized tooth, the distal root of which shows where the impacted tooth has pressed against it. Not only has it done this, but it has also caused absorption of a portion of the root until the pulp-canal within the root is fully exposed, and it must have caused neuralgia in these parts. The roots of the impacted teeth have a slight curve inward at their points; the concavity fits immediately over the inferior dental nerve, and more than likely caused pain by pressure.

The ends of the roots are not fully formed, the apical openings being large. It will be noticed that the roots of the teeth in this jaw are longer than usual, the cuspid passing below the nerve and to its outer side.

Slide No. 31 represents another impacted third molar, situated on the inner side of the jaw, and pointing slightly downward. The distal root of the second molar is slightly absorbed. On uncovering the tooth and taking it from its bed, it was found to be incased in a thin shell of bone as though the dentinal sac had ossified separately around the tooth. The inner portion of this shell is still in position; the nerve, with its accompanying structures, is seen passing into the inferior dental foramen immediately against the shell, and has the appear-

ance of being flattened out; it divides and sends a branch around the internal half of the shell. Here again must have been an obscure cause of neuralgia.

Slide No. 32 is made from the left and right halves of a lower jaw, one showing the internal surface of the right half, while the other shows the external surface of the left. In that of the right we find the roots of the third molar curved backward. It is also enlarged by an abnormal deposit of cementum until the independent character of each root is lost. This, I think, has been caused by the vascular excitement localized in these parts.

Not only have the roots become enlarged, but the surrounding bone has become much thickened, and the cancellated tissue of the jaw has lost its fine structure and become more solid.

It would have been impossible to have extracted this tooth without breaking quite a large piece from the inner side of the jaw. A fracture at this point often produces serious results; the mylohyoid artery is liable to be lacerated, or even broken, and the hemorrhage is difficult to control. The consequent inflammation will often cause the lack of free movement of the jaw. It also interferes with deglutition, speech, etc., and even the glottis may become closed.

The left half of this jaw shows an impacted tooth pushing directly against the tooth in front of it. The roots of this tooth have become much enlarged by deposit of cementum. The surrounding bone is also thickened and more compact.

Slide No. 33 is taken from a section made transversely through the lower jaw at the mental foramen of each side. On the left side the cortical U-shaped portion of the bone and the cancellated tissue is in about a normal condition, while that of the right side, the cortical U-shaped portion, and the cancellated tissue have changed. The former has thickened and become more dense, while the latter has become filled with secondary bone-deposit. The only reason that can be given for this difference is that all the teeth on the right side were in good condition, while on the left side the first molar had been much diseased, causing an inflammation of that side of the jaw, the vascular excitement having caused the activity of the osteoblasts of this region to build this bone.

DISCUSSION.

Dr. B. Alex. Randall said that as the antrum of Highmore is the meeting-ground of laryngologists, ophthalmologists, dental and general surgeons, it must be viewed in many different aspects. It is from the ophthalmic point that he had generally studied it, as not infrequently affections manifested about the orbit have their origin in antral inflammation. Such lesions may undergo spontaneous cure; and although the position of the antral opening is highly unfavorable to drainage, if men were always as upright creatures in fact as in theory, it must not be forgotten that we often lie down. Drainage of the sinus can often be very satisfactorily achieved, even when the opening is considerably narrowed by swelling, if the head be properly inclined. Dr. Randall has himself occasionally indulged unwisely in acute inflammation of the antrum, and discovered accidentally the cause of his severe "toothache" by stooping over and draining away, with immediate relief, nearly a dram of yellow serum. The same result may often be gained with patients, Bayer and others recommending that the head be hung down by bending over the back of a chair while the hands rest on the seat.

The natural opening of the antrum may be closed by inflammatory thickening, either of its own margins or of adjacent parts; and the accessory opening often present may be equally occluded. Cocain-shrinkage may only facilitate access, not affording drainage; but the syringe-canula may in many cases be introduced into the semilunar hiatus by pressing aside the valve-like tissues, and with it this cavity or the frontal sinus may be washed out.

Dr. G. G. Davis said that a debt of gratitude is due to anyone who will throw light upon disease of the various sinuses that enter into the nasal cavities, as the diagnosis is often very obscure and the treatment exceedingly difficult. This will readily be appreciated when one considers that a discharge coming from the middle meatus may start from the anterior ethmoidal cells, from the frontal sinus, or from the antrum of Highmore. They all open at practically the same spot, the infundibulum. One has to rely upon collateral symptoms to establish a diagnosis. Even after an approximate diagnosis is made, in some cases it is almost impossible to apply any treatment. Take a disease of the ethmoidal cells as an example. It has been suggested by Bryan that they should be curetted away. Bosworth advises the use of the dental drill. When it is considered that the ethmoidal sinuses are covered by a thin scale of bone, separating them from the brain, and when it is remembered that on their outer side is a membrane of bone which is likewise extremely thin and which constitutes part of the inner wall of the orbit, and that the lachrymal canal also passes through the bone, one can appreciate the dangers of attempting promiscuous drilling in this situation, particularly when the least disturbance produces such a flow of blood as to prevent one from effecting these procedures under direct ocular inspection. In the treatment of diseases situated in the anterior or middle ethmoidal cells and frontal sinus, Dr. Davis suggested the following procedure for trial: In endeavoring to cleanse the frontal sinus, he had the patient placed in the supine position, with the head hanging over the edge of the table. Any solution injected through a canula introduced into the infundibulum will trickle by gravity down into the sinus, particularly if the fluid employed be an effervescent one like hydrogen dioxid, for example. A far better cleansing can be secured through the various cells in this way than if the patient be in a sitting position with the head upright. Of course, this is simply a moderate and not the most radical means of curing these affections, but it is one that seems worth trying.

Dr. Cryer, in closing the discussion, said that during his studies he had not found a normal skull in which it would be possible to pass a sound, but he had seen many pathologic skulls in which this could be done. Speaking of drainage, he said that if a person should stand on the head or lie on one side the antrum would, of course, drain itself, but in the erect position or lying on the back there could be no drainage, as the normal opening is at the anterior superior angle of the sinus. Occasionally the opening is in the roof of the sinus, and there are cases in which it passes into the infundibulum of the frontal sinus.

THE TREATMENT OF PERNICIOUS ANEMIA.

ALFRED STENGEL, M.D.

[Read April 8, 1896.]

Two years ago I had the honor of reading before the Society a paper on the "Nature, Diagnosis and Treatment of Pernicious Anemia," and of alluding to certain cases that had come under my observation.‡ I wish now to add the experience of the last two years and to discuss further measures of treatment, especially that which has recently become popular—the administration of bone-marrow.

It seems necessary to me now, as it did two years ago, to consider, first of all, the cases which are to be classified as pernicious anemia, and to discuss the nature of the affection. There is little that I wish to add to what I said then regarding the nature of the disease. The weight of opinion has steadily moved toward a general acceptance of the view that pernicious anemia is a hemolytic disease. Aside from the fact that experimenters have produced anemic conditions in animals similar to the disease as it occurs in man, I would again allude to the pathologic lesions discovered in the disease, and especially to the abundant deposits of pigment found in the liver, the spleen and other organs; to the occurrence of icterus, and of pathologic pigmentation of the urine; and to the less certainly significant discovery of peptonuria and of excess of uric acid in the urine.

As far as the blood-making organs are concerned, the evidence points to inordinate activity rather than to diminished function. We may assume, therefore, that there is active hemolysis rather than faulty hemogenesis in this disease. In my previous discussion I held the view that while the disease is essentially hemolytic in nature, it might still be proper to speak of deficient hemogenesis in the sense that a certain amount of hemolysis would exercise no decided influence upon the condition of the blood in healthy persons, while in those of lowered blood-making power progressive deterioration of the blood would ensue. In proof of this view we may recall to mind the fact that similar diseases act most dissimilarly in different persons, and that while in one but a moderate anemia follows a gastro-intestinal or other disorder or a loss of blood by hemorrhage, in another there is extreme debility and deterioration of the blood. Cases might then occur in which the ordinary wear and tear of life, in a person of deficient hemogenetic power, would lead to the clinical picture of pernicious anemia. Such cases, however, do not seem to occur, as far as any observations now at hand would indicate. In all cases in which studies of the pathology of the disease have been made, there is evidence of active hemolysis. Therefore we are led to the belief that in all cases of pernicious anemia the condition of the blood results from active blood-destruction, which is either so decided that normal hemogenesis cannot cope with it, or decided enough to overbalance the deficient hemogenesis of the particular individual.

The essence of the disease is, therefore, blood-destruction. The seat of

‡ *Therapeutic Gazette* for June, 1894.

this remains doubtful, though recent experiments and investigations have partially cleared the way for a fuller knowledge. The occurrence of hemoglobinemia, which is occasionally observed, and of icterus, which is an almost constant symptom, led at first to the assumption that there is in this disease, as well as in other diseases attended with so-called hematogenous jaundice, widespread blood-destruction in the general circulation. Recent investigations, however, leave no room for doubting that such jaundice can have only an hepatic origin. Experimenters are unanimous in the view that blood-destruction leading to the formation of hematoidin, bilirubin or hematoporphyrin and consequent jaundice, can have its final stage only within the liver. The preliminary separation of hemoglobin or hematin may occur in the general circulation or in any local part, as in the portal area, but the final change occurs in the liver. Whatever view we may hold as to the original seat of the hemolytic changes, we cannot deny the rôle of the liver as a contributing organ in the final changes brought about in the circulating blood.

Quincke, Mott, Peter, Hunter, Griffith and Burr, and others have investigated the pigment-deposits found in pernicious anemia, and have in particular called attention to the iron-containing pigment of the liver and spleen. Hunter in some of his earlier studies found that there was evidence of blood-destruction in the spleen, in the gastro-intestinal tract, and in the bone-marrow, and he regarded the relative importance of these organs as falling in the order in which they are named. More recently he has inclined to the view that the spleen is the all-important seat of hemolysis as far as pernicious anemia is concerned. This view, it seems to me, is a retrograde one, and based, as it largely is, upon the occurrence of large amounts of iron-containing pigment in the spleen, rests upon uncertain ground. Pathologists have long maintained, and with apparent justification, that the spleen is a filtering organ, as are the lymphatic glands. The presence of abundant pigment, and particularly that in the lymphoid nodes, where it is most abundant, might, therefore, readily occur in cases of blood-destruction taking place in any part of the circulation. This is well illustrated by the enlargement and pigmentation of the spleen, which occur in experiments in which blood-destruction is brought about by injection of distilled water or direct transfusion of blood into the circulation. The existence of pigment in the spleen is, therefore, no indication that it originated there; neither is the presence of brownish coloring-matter resembling hematoidin or bilirubin to be looked upon as of splenic origin.

On the other hand, there is evidence of a clinical kind that blood-destruction may have a gastro-intestinal origin. I need only allude to the constant anemia of diseases of this part of the body, to the occurrence of severe anemia in cases of gastric atrophy, in some cases of gastric carcinoma, ulceration, etc., and to the clinical evidence that has been brought by those who favor the view of Sir Andrew Clark regarding the intestinal origin of chlorosis. I may further allude again to the cases of Sandoz, Jürgensen, Meyer, and others, in which severe anemia was rapidly cured by lavage or removal in other ways of bacteria and noxious agents of other kinds from the gastro-intestinal tract. It seems likely that the first stages in the process of hemolysis begin in the capillaries of the gastro-intestinal tract, and that the final change, with the formation of iron-containing pigment, occurs in the liver. That this pigment is not brought to the liver for the purpose of elimination with the bile (as would at first sight seem probable) is proved by the fact that the bile normally contains but a trace of iron. It is more probable, therefore, that the iron-pigment is formed in the liver and subsequently removed to parts where it may be utilized or excreted. The former purpose is probably carried out in the bone-marrow and spleen, the latter in the gastro-intestinal tract and to a very small extent in the kidneys.

I believe, then, that pernicious anemia is a hemolytic disease, in which hemogenesis may or may not be equal

to the normal, though always insufficient to cope with the hemolysis; that this hemolysis is originated in the gastro-intestinal capillaries, and is dependent upon poisons generated or at any rate absorbed from that tract; that the final splitting-up of the blood coloring-matter occurs in the liver; and that the hemosiderin is subsequently removed to the spleen and bone-marrow (perhaps for purposes of hemogenesis) and to the intestines and kidneys (to be excreted).

As far as our knowledge of the disease extends to-day, it is but a symptomatic disorder of the blood, and not a disease in the strict sense. It matters not whether a cause is discovered or not; whether there be gastro-intestinal lesions, parasites in the intestines, pregnancy or parturition, or no discoverable cause—the resulting condition of the blood, the symptoms, the course and tendency to a fatal termination, and the general post-mortem lesions are the same; and I can therefore see no justification for setting apart a group of cases as true Addisonian pernicious anemia, and calling all those in which causes are found severe secondary anemia. One set of cases is as progressive and as pernicious as the other, and the deduction seems obvious that the apparently causeless forms are such as have baffled our minuteness of examination or our methods. I have seen cases of pernicious anemia dependent upon or associated with gastric atrophy, gastric carcinoma, gastritis with polypoid growths, parturition, and ankylostomiasis (in this case the blood only), and have been unable to see differences of a characteristic kind from other cases in which the disease appeared wholly causeless. All cases of severe anemia—or, as it has been suggested to call such cases, of *cachemia*—in which maximum degrees of oligocythemia, with microcytosis, macrocytosis, and poikilocytosis, occur, in which nucleated red corpuscles are discovered, and the hemoglobin is relatively excessive or at least approximately equal in percentage to that of the corpuscles—all such cases may be designated as pernicious anemia. In the treatment I believe it is wise and proper to regard all such as cases of hemolytic disease of gastro-intestinal origin.

The treatment may be divided into three heads: first, "specific" treatment; second, the treatment directed to altering the general conditions of the circulation; and third, treatment of the intestinal conditions or of underlying affections which influence unfavorably the progress of the case or perhaps act as the immediate causes of the disease.

The "specific" treatment of pernicious anemia consists in the use of three remedies—iron, arsenic and bone-marrow—and I shall consider them in this order, as it is the order in which they have been brought before the notice of the profession.

Iron.—The value of iron in anemia is unquestioned. It may be true, as Bunge and his pupils maintain, that only a comparatively small proportion of the iron administered is absorbed; but whether this is the case or not, clinical experience allows no question regarding the value of this drug in various anemic conditions. It is particularly valuable in cases of chlorosis and in the ordinary anemia of debility. Its use has been advocated in pernicious anemia, and in certain cases good results have been obtained. In most cases, however, iron is of little value, and I am positive that I have seen it contribute to the patient's discomfort and unfavorably influence the progress of the disease, when it was administered during the active stage of pernicious anemia. In convalescence it is a valuable remedy; and though the view of Stockman, that iron is practically a necessity at this stage, is probably too radical, there is no doubt that the combination of iron with arsenic or other treatment is most desirable during the convalescence of this disease. It may be administered in the form of Blaud's pill, or as iron albuminate, or in the form of one of the physiologic preparations now in the market. The latter, I believe, have no special advantage over some of the inorganic preparations, excepting that they may be more acceptable to the stomach and not so constipating.

Arsenic was introduced by Isnard in 1865, but was more particularly commended by Bryam Bramwell, to whom the credit of its introduction is due. There is no doubt that in many cases of pernicious anemia this remedy is little short of a specific. It is to be recalled, however, that pernicious anemia, like other anemic diseases, is liable to curious and unexpected improvements, and Laache has pointed out that spontaneous recovery is occasionally met with. With the knowledge of these facts before us, however, we may assert that the remedy has really power to combat the progress of the disease and perhaps to effect a cure. Regarding the manner in which arsenic acts, little can be said. It may be that in some way it prevents intestinal putrefaction and thus limits the formation of the immediate cause of the disease; again, it may exercise a direct influence upon the corpuscles of the blood, increasing their resisting power; or, in the third place, it may increase the hemogenetic function. I am inclined to believe that it acts in some way to prevent blood-destruction and possibly at the same time to regulate the function of the bone-marrow and other blood-making organs. If it stimulated the latter without influencing blood-destruction, we might expect a sudden increase of nucleated corpuscles and other immature forms of erythrocytes after its administration, but such does not occur. There is a progressive tendency on the part of the blood to the assumption of a more healthy appearance in size and shape and other features of the corpuscles; so that it would seem as if blood-destruction were progressing less rapidly and the hemogenetic organs were influenced to restore the normal blood-formation. This, however, is largely speculation, and perhaps after all unimportant. The clinical fact remains that this remedy influences favorably the course of the disease. Whether the improvement is a permanent one or not cannot be positively asserted. Cases of apparent cure after intervals of three, five or more years have been reported; but on the other hand temporary improvement has been noted by Pye-Smith, Mackenzie, Barrs, Russell, and others; while in many of the cases in which definite cure was supposed to have occurred, the patient has passed out of observation. My own experience would indicate that in many cases in which the patient has been apparently cured, the disease has in reality been only superficially influenced, as may be seen from the reports of some of the following cases, in which, though the patients presented a healthful appearance and believed themselves to be well, the blood-examination showed a continuance of the disease. Such cases are prone to recur, and Mackenzie has specially called attention to a form of relapsing pernicious anemia. In this form, I incline to believe, there has never been a definite cure, the intervals representing periods of apparent recovery, though blood-examination might have shown a continuance of the disease.

The administration of arsenic in cases of pernicious anemia is a matter of considerable importance. In all cases it should go hand in hand with repeated examinations of the blood, and should be regulated by the response or tolerance of the patient. Sometimes improvement is at once apparent, and only moderate doses are required; in other cases excessive doses, even to the production of toxic symptoms, may fail to influence the course of the disease. Ordinarily Fowler's solution, administered by the mouth in doses of from five to twenty drops three times daily, proves sufficient. If the dose has been increased to twenty drops without noticeable change in the condition of the blood, great care should be exercised in the further increase of the dose. In particular it is to be remembered that gastro-intestinal disturbances may be provoked and may cause rapid loss of ground. When this is feared, it is well to administer the remedy hypodermically. Five or ten minims of Fowler's solution, diluted with an equal quantity of water, may readily be administered in this way, and I have never found it in the least irritating. The results are sometimes quite striking, in contrast with the effects of the administration of like amounts by the mouth.

The following cases illustrate the value of arsenic in the treatment of pernicious anemia and at the same time illustrate the point which has already been brought out—that apparent cure does not imply real recovery, and that it is imperative to continue the treatment without interruption for a long time, perhaps for years, despite the most encouraging appearances.

Case I.—A. T., aged sixty-three, single, presented herself at the Howard Hospital in June, 1895, giving the following history: She was a healthy girl and woman until five or six years ago, when she began to suffer from pain in the chest, weakness, and disturbance of stomach. There was increasing flatulence and repeated vomiting, which finally became almost constant; the appetite was poor; there were frequent headaches; but the bowels remained regular. Her sleep became disturbed, and she grew more and more weak. About six months before admission she began to be pale and noticed that she was growing short of breath; cough became a distressing symptom, and so continued, with pain in the chest.

On admission her eyesight was not good, and she complained of a feeling of pins-and-needles in her feet and ankles; brain and memory were active; extreme weakness and pallor were noted, the color being yellowish; the conjunctivæ were somewhat icteric; the feet were swollen, and pitted on pressure; the tongue was coated; the lungs were healthy; the heart-sounds were very feeble, but no murmur was audible; the abdomen was moderately distended and tympanitic; the knee-jerks were a little increased, but there was no loss of sensation. Examination of the blood showed 1,400,000 red corpuscles to the cubic millimeter and forty per cent. of hemoglobin. The stained preparation of the blood showed marked irregularity in the size of the red corpuscles and some irregularity in shape; there were nucleated red corpuscles, both small and large.

The patient was given Fowler's solution in ascending doses.

On July 12th, the patient returned, feeling much better; there was almost no swelling of the legs; the appetite was good, but flatulence was still present and the tongue was coated; the knee-jerks were still excessive, but the sensory symptoms were less prominent; the lips were becoming red. The red corpuscles numbered 1,615,000 to the cubic millimeter; the hemoglobin-estimation was 45 per cent. Examination of her eyes by Dr. Posey showed some evidences of old hemorrhages.

On July 26th she had improved remarkably in every way, and felt that she was quite well. The red corpuscles numbered 2,560,000 to the cubic millimeter, and the hemoglobin-estimation was 62 per cent.

During August and September the patient was quite comfortable, but in the latter month she had an attack of bronchitis and relapsed. On October 8th she returned, with 50 per cent. of hemoglobin and 2,000,000 red corpuscles to the cubic millimeter. She was again ordered to take arsenic, which lately she had neglected, and during November she improved considerably. In December she was careless of her medicine and was considerably exposed; so that by the end of the month she had again relapsed.

In January, 1896, I saw her at her home, and found that she had acute lobar pneumonia, the base of the right lung being consolidated. There was considerable edema in the other parts of the lung. Treatment with arsenic was continued vigorously, and strychnin and other stimulants administered. After three weeks she had practically recovered; and in March she was again in her normal condition, though still pallid and yellowish, short of breath on slight exertion, and troubled with slight cough. Her hemoglobin-estimation was now 45 per cent.

Case II was under the care of Dr. George Woodward, with whom I saw her in August, 1894. The history was as follows: H. M., aged seventy-seven, born in England, came to America when fifty-three years old. She was married at twenty-one years, and bore twelve children; there had been no miscarriages. Her previous health had been poor for a number of years, dating from

an obscure illness. She had been asthmatic for twenty years. In 1890 her husband died; the loss greatly affected her, and her memory and general health began to fail. In 1891 she had a severe attack of influenza, and in 1892 another. Since the latter date she had scarcely been well at all. She came under Dr. Woodward's care in April, 1894, with dyspeptic symptoms. She became profoundly anemic, and there was a murmur over the base of the heart; the lungs were normal; the urine was normal; the pulse generally was about 60; the respirations 22. Her anemia and debility increased progressively, so that she was in bed the greater part of the day, and finally in July was entirely bed-ridden.

In August the following notes were made: "The patient is apparently well nourished, having lost little if anything in weight. Her skin is pallid—ofttimes yellowish; and the conjunctivæ are pearly blue. No arcus senilis is present. There is some edema of the ankles, and at times this becomes marked. There are discolorations of the skin along the tibiæ, and spots of ecchymosis on the forearms. The superficial vessels are not prominent. The pulse is 60, somewhat irregular and collapsing. The area of cardiac dulness is increased, and a double murmur is heard over the root of the aorta. The respirations number from 26 to 28, and are regular and sometimes asthmatic. There is no cough or expectoration. The voice is clear. Her stomach is easily disturbed; but the bowels are regular. The liver and spleen are of normal dimensions. There is no fever. The urine is light-colored and normal. There has been no pain at any time, but the patient complains of numbness of the legs. The knee-jerk is present, though greatly lessened. The thyroid gland is enlarged."

Examination of the blood showed 2,000,000 red corpuscles to the cubic millimeter and 25 per cent. of hemoglobin; the drop as it exuded from the finger was light-colored, and the clear plasma separated readily from the corpuscles. Examination of the stained preparation showed abundant megaloblasts and normoblasts, marked poikilocytosis, and irregularity in the size of the corpuscles.

Arsenic was administered in ascending doses by the mouth, and was continued with little interruption.

In October, 1894, I saw the patient again. She had been out of bed for some weeks and was apparently quite well. Her memory was still poor, but she considered her physical health normal. Examination of the blood, however, showed only 1,800,000 red corpuscles to the cubic centimeter, and 65 per cent. of hemoglobin. She was directed to continue the arsenic.

In February, 1896, I saw her for the third time. She had taken Fowler's solution almost without interruption since October, 1894. She now seemed quite well—in fact, more robust than most persons of her age, now over seventy-eight. There was still a cardiac murmur at the aortic region, systolic in time, and a less distinct diastolic murmur. The hemoglobin was between 95 and 100 per cent. (The reading was probably a little high, as some congestion was produced to obtain the drop of blood.)

Bone-marrow was introduced as a remedy in the treatment of pernicious anemia by Fraser, of Edinburgh, in 1894, and has since been used by a number of clinicians, who have reported variable results. The use of bone-marrow can scarcely be said to rest upon a scientific basis, and some have objected to it upon the ground that the bone-marrow is in nowise an organic secreting tissue, and that in consequence little good need be expected from its administration. It has been held that, as the formation of the blood, with which the bone-marrow is concerned, is a process of cell-multiplication rather than a secretion, the administration of extracts could scarcely be expected to give results. On the other hand, it may be said that our knowledge regarding the function of the bone-marrow, and particularly its relation to the fluid elements of the blood and to the general metabolism, is so meager that we are not permitted dogmatically to assert that it is devoid of action of the chemical sort and the disproof or proof of

the value of this remedy must therefore rest for the present upon clinical experiment rather than speculation. Fraser, Danforth, Drummond, Barrs, Hamilton and others have reported cases in which bone-marrow seemed a useful remedy, and I shall record a case of which the same could be asserted. On the other hand, J. S. Billings, Jr., Stockman and others have found it useless in a small number of cases, and I shall report a case in which the same observation was made. It may be said regarding the latter that the condition of the patient was extremely unfavorable when the remedy was first administered, and the surrounding conditions did not permit of the most accurate conduct of the treatment.

Some of those who have objected to the use of bone-marrow on theoretic grounds have sought to explain its occasionally apparent usefulness by the statement that it contains sufficient iron to prove beneficial in anemia. It would seem scarcely necessary to dispose of this view by accurate analysis, as the amount of bone-marrow administered has always been relatively small and therefore the amount of iron consumed in this way must be considerably below that ingested with the food; but Stockman has taken the trouble to analyze the marrow. Taking the yellow marrow of three specimens from different animals, he found in one (26 gm.) 0.001 gm. of iron; in the second (20 gm.), slightly less; and in the third (100 gm.), 0.0025 gm.; therefore an ounce of yellow marrow contains about one sixty-fifth of a grain of iron. In analyzing the red marrow of the calf, he found that 40 gm. contained 0.0035 gm. iron, and 26 gm. contained 0.002 gm.: or an ounce of the marrow contained about one-thirtieth of a grain of iron. These facts are sufficient to show that the marrow does not act by virtue of the iron contained.

Regarding the variability in action in different cases, while it must be admitted that the entire number reported has been insufficient to warrant positive conclusions, I have been inclined to believe that much depends upon the preparation of the marrow, and in one of my own cases a glycerin extract was employed without striking effect, while the raw bone-marrow acted immediately, to all appearances. It was the failure of the glycerin extract made from calves' ribs that led me to turn to raw bone-marrow. I had previously used the marrow plain, but had found that the patients soon rebelled and that the stomach became unretentive. I therefore looked for a more acceptable form and found that by taking the red marrow of the tibia of the calf, mixing it with an equal quantity of glycerin, and rubbing the mixture up in a mortar, a preparation could be made, the taste of which was pleasant, and which could be eaten upon bread without danger of disturbance of the stomach. The preparation may be made a little more fluid by the addition of claret or port wine; but I prefer the simple glycerin jam. From one to two ounces of this may be administered during the day. I have always been careful to get marrow which was quite red, as this is certainly the functionally active sort.

I shall now report two cases of advanced pernicious anemia in which bone-marrow was used. In the first, raw bone-marrow was employed for a short time, but not long enough to determine its value; later, arsenic with a glycerin extract of bone-marrow was given without notable effect; finally the marrow-jam was administered when the patient was almost moribund and when large doses of arsenic had been given continuously without any effect. Though the arsenic was continued in small doses at the same time, the immediate and striking improvement which occurred seemed fairly attributable to the bone-marrow. In a subsequent relapse bone-marrow was given again, but without effect.

In the second case the patient was far advanced in the disease when the treatment was begun, and the bone-marrow exercised no influence.

It is obvious that deductions cannot be drawn from a single case of improvement or of failure. In the first case the sudden and striking improvement might have been co-incident, though the manner in which the stomach became retentive and the general conditions improved

was most suggestive. The failure in the second case may have been due to the advanced stage of the disease, and certainly the conditions under which this patient was treated did not permit of the same accurate regulation and observation as in the first case.

Case III.—H. S. B., aged thirty-nine, presented a family history that had no bearing on his case. He had been a strong, healthy man, working hard all his life. In 1886 he had malaria while living in Delaware. He was accustomed to the use of liquor and tobacco in excessive quantity. There was no venereal history. In 1891 he had an attack which was considered influenza, and he asserts that he never entirely recovered from this. The fatal illness began in June, 1893. He had been working hard and drinking considerably, taking one glass of whisky before breakfast every morning. He first had diarrhea and began to become pale, his strength failing and his memory growing poor. He became a patient of Dr. A. J. Bevan, with whom I saw him first in September, 1894; he was then extremely pallid and of a yellowish hue, puffy beneath the eyes and about the ankles; dyspnea was pronounced, and he complained of passing more than the normal quantity of urine and of being disturbed at night to empty the bladder. Examination showed his lungs to be normal. There was a systolic murmur at the pulmonary region, and less distinctly at the apex. Examination of the blood showed 1,150,000 red corpuscles to the cubic millimeter and 40 per cent. of hemoglobin; there was no leukocytosis. The specimen of stained blood under the microscope showed typical microcytes, macrocytes, and nucleated red corpuscles. Poikilocytosis was not pronounced.

The patient was given large doses of arsenic, at times enough to produce toxic symptoms, and improved somewhat. Later he ate raw beef bone-marrow until he sickened. During this period there was considerable improvement in every way. He next took a glycerin extract made from calves' ribs, together with arsenic, and continued to improve, though slowly. He was quite comfortable during the winter of 1894–95. In February, 1895, he sustained an injury and then had an attack of bronchitis, in consequence of which his condition became worse, and in April, 1895, he was again quite weak and had considerable dyspnea, his blood showing 1,000,000 red corpuscles to the cubic millimeter, and 40 per cent. of hemoglobin. He was treated with arsenic, but took his medicine irregularly and grew steadily worse.

In June, 1895, he was admitted to the Howard Hospital in an extremely weak condition. The blood showed 600,000 red corpuscles to the cubic millimeter and 25 per cent. of hemoglobin. There was marked poikilocytosis, many large nucleated corpuscles, and some hemoglobinemia. He became wildly delirious and had to be restrained in bed; his stomach refused every form of food. Arsenic had been given in large doses. Bone-marrow jam was now administered in doses of one, two, or three teaspoonfuls, four to five times daily, and was well retained. At once there was manifest improvement, and after a month the patient was seemingly as well as ever. The blood, however, showed only 1,500,000 red corpuscles to the cubic millimeter and 45 per cent. of hemoglobin.

The man left the hospital, refusing further treatment. His condition again grew serious in the autumn, and in December, 1895, he stopped work. This attack was gradual in onset, the patient growing weaker and weaker. He entered the University Hospital June 8, 1896, presenting the characteristic features of marked pernicious anemia. His color was yellowish, the subcutaneous tissues were flabby, there was puffing beneath the eyes, and the vessels of the neck throbbed. There were venous and arterial murmurs. The heart was somewhat increased in size, and there were murmurs at the apex and base. The blood showed 1,100,000 red corpuscles and 3,125 white corpuscles to the cubic millimeter, and 20 per cent. of hemoglobin; there were nucleated red corpuscles, microcytes, and macrocytes. The urine was light-colored, with a specific gravity 1.005, and contained no albumin or sugar. The man was given

the preparation of bone-marrow he had before received, but there was no improvement noticeable; on the contrary, his condition deteriorated from the first. For several days this was gradual, then a sudden change set in, and he died after a few days.

The autopsy showed characteristic changes of pernicious anemia—extreme fatty degeneration of the organs, violaceous color of the bone-marrow.

Case IV.—Mrs. B., aged about sixty, had been healthy the greater part of her life. In childhood she had had measles, but no other illnesses. She was married at twenty-four, and had four children; there had never been any difficulty in or after any of her labors. She had had hemorrhoids when she was fifty, and had lost some blood now and then since that time. Her health was good until January, 1895, when her daughter noticed that she was growing mentally slow and somnolent, and she complained of numbness in the feet. Dr. A. J. Bevan was called, and found her pale and greatly weakened; dyspnea followed exertion, and several times she had slight syncopal attacks.

I saw her with Dr. Bevan somewhat later, and found the conditions noted. Her color was rather yellow than white, and the conjunctivæ were yellow. She was mentally slow and apathetic, taking little interest in her surroundings.

The drop of blood as it flowed from the finger was extremely thin and presented the characteristic appearance of meat-washings. The red corpuscles numbered 1,142,000 to the cubic millimeter, and the hemoglobin-estimation was 25 per cent. of the normal; there was no leukocytosis; the corpuscles were irregular in shape and size, and nucleated forms, both large and small, were present in rather considerable abundance.

The patient was put upon treatment with bone-marrow jam, but she steadily grew worse; arsenic was then given in conjunction, but despite the treatment she sank and died.

There was no autopsy.

It would be a grave error to suppose that the treatment of pernicious anemia was satisfactorily fulfilled by the administration of one of the "specific" remedies and the regulation of the ordinary details of hygiene. There are other and important indications, and at times I am sure that arsenic or bone-marrow would prove of little use unless auxiliary measures of treatment were instituted at the same time. First of all, I believe it is necessary to regulate the circulation and to consider the quantity of blood. Much remains to be learned regarding the circulation of the blood in the tissues, though we have learned enough to know that frequently there is stagnation of corpuscles, and that therefore but a part of the blood-corpuscles may be circulating and functionally active. It is of prime importance, therefore, in the treatment of a severe anemic disease, such as the one under consideration, that all of the available corpuscles —none too many at best—be brought into the circulation and rendered useful. For this purpose massage is of great value. It has been pointed out by a number of observers that the corpuscles of the hand held in air number from five to ten or fifteen per cent. less per cubic millimeter of blood than do those of the toe or other dependent parts. In my own experiments in this direction I have found a uniform excess of from eight to ten per cent. in the blood of the toe, and practically a similar excess of hemoglobin. Recently J. K. Mitchell demonstrated the advantage of massage to equalize the circulation, and found the number of corpuscles to be thereby quite uniformly increased, even in the most pronounced forms of anemia. It is therefore clear that in the treatment of pernicious anemia, rest in the horizontal position (which will favor the maximum effect for the cardiac action) and massage (which will return to the circulating blood and to usefulness a large proportion of corpuscles) are of considerable importance.

Next to these measures it is of importance to consider the question of quantitative anemia. Unfortunately there is no clinical method available for the determination of the quantity of blood; but the difficulty with which the blood flows from incisions, and the general desiccated condition of the patients in

some cases, particularly towards the end of the disease, leave little doubt that reduction of the blood-mass plays some part in the pathology of the affection, though perhaps not so frequently as is the case in certain other forms of anemia. Near the end of the disease the patient's condition may be endangered by this reduction in the quantity of blood and the consequent failure of adequate circulation, more than by deterioration in the quality of the blood, and measures are called for which will increase the blood-mass. Injections of large quantities of water or of weak saline solution into the large bowel have the double advantage of stimulating peristalsis and thus of improving the general circulation, and perhaps at the same time removing from the system noxious substances, and also (a greater advantage) of supplying a certain amount of liquid to the empty vessels. In one case of pernicious anemia which has come under my observation, there was rapid and unmistakable improvement from the first administration of colonic douches; and while to an extent this improvement was ascribable to improvement in local conditions of the colon, to a large degree it was, I think, owing to absorption of liquid from the bowel.

In certain cases it may be of advantage to substitute for colonic injections hypodermic or intra-venous injections. The latter form commends itself particularly in cases in which the circulation has become considerably enfeebled, and absorption of the water is therefore unlikely to occur speedily from the subcutaneous tissues or from the bowel. These methods of improving the circulation by increasing the quantity of the circulating medium are, I believe, of incalculable advantage in many cases, and failure of arsenic at times is due to the fact that such measures have not been taken to improve the circulation and make the further action of the specific remedy possible.

Brackenridge, von Ziemssen and others have recently advocated transfusion with defibrinated blood, but there seems little reason to believe, in the light of our present knowledge, that this procedure has any advantage over simple saline injections. The corpuscles introduced with the blood probably undergo destruction rapidly, and it is doubtful whether they are functionally active even for a short time.

The treatment of the underlying affections which perhaps act as the immediate causes of the disease, or which influence unfavorably the progress of a case, comprehends attention to every detail of the patient's physical condition. In particular, however, it is important to consider the question of intestinal antisepsis and other treatment directed to the gastro-intestinal tract. The diet should be so selected that the food may be readily digested and that it may undergo as little decomposition as possible. Liquid food, and particularly milk, will prove most useful. In many cases it is of advantage to have the foods predigested. The quantity at a meal should be small, but food should be given at frequent intervals. Hydrochloric acid and bitters may be used to aid digestion.

Finally, if our speculations regarding the nature of the disease are correct, and the prime cause is gastro-intestinal fermentation, it should be possible to influence favorably the course of the disease by measures directed to the prevention of fermentation in this tract. This has undoubtedly been done in a number of cases. I have already referred to the satisfactory results obtained from colonic lavage in one case; and although this was partly due to the absorption of liquid from the bowel, it was due also, to some extent, to the mere cleansing effect of the injections. Certainly this method of treatment offers sufficient hope of beneficial results to warrant its more extended introduction. With careful manipulation it is easy to flush the entire colon with water, and probably also a large part of the small intestine; and gastric lavage may be practised with but little inconvenience. This treatment is particularly advisable in cases in which alternating constipation and diarrhea or excessive vomiting with a fermenting condition of the vomited materials gives evidence of the gastro-intestinal disturbance.

Intestinal antiseptics, such as salol,

hydro-naphthol, and the like, have been employed, and Gibson in particular has reported good results from this mode of treatment. The failure of intestinal antiseptics to control fermentative processes in other affections, however, makes their use less advisable in pernicious anemia than theoretic deductions would lead us to expect.

The treatment of pernicious anemia in association with intestinal parasites is clear and direct. It consists in the administration of remedies such as thymol, santonin, or other anthelmintics, and the subsequent use of tonics, arsenic and iron.

In conclusion, I would repeat that pernicious anemia is probably a hemolytic disease, of gastro-intestinal origin, dependent upon fermentative or putrefactive changes occurring in the stomach and bowels; that the treatment calls for remedies of a specific nature and for measures directed to the improvement of the general circulation and to the prevention of gastro-intestinal fermentation. In the present state of knowledge, arsenic is the remedy on which most reliance may be placed. Bone-marrow has seemed to give results in a number of cases, but these have not yet been sufficient to furnish ground for an accurate opinion.

DISCUSSION.

Dr. James Tyson said that, notwithstanding the large amount of information that has been yielded by modern studies upon this subject, pernicious anemia must still be regarded as a *terra incognita*. It was formerly held that pernicious anemia is an affection of the blood-making apparatus, but most clinicians have come gradually to the view held by Dr. Stengel, that it is an hemolytic affection. Among reasons for this may be mentioned, in the first place, the experiments of Hunter, so amply confirmed by those of Griffith and Burr, showing the deposition of iron in the liver, representing there the destroyed corpuscles. Secondly, the condition of the blood in which there is a comparatively large amount of hemoglobin, with a proportionately small number of corpuscles, pointing to a destruction of corpuscles, leaving a relatively large quantity of hemoglobin in solution. A most important reason, however, is the etiologic factor. The frequent association of the affection with long-continued, unyielding, gastro-intestinal disease is attested by the experience of many. Dr. Tyson referred to the case of a man who was a confirmed subject of indigestion, which was ascribed to all sorts of causes. At the end of twenty-five years, he fell into a condition of pernicious anemia. The hemolytic nature of the disease is also attested by therapeutics. While it is true that it is not known in what way arsenic acts, it is known that it does good, and it must act in some way as a blood-builder. Dr. Tyson had personal knowledge of at least one case in which the symptoms of pernicious anemia disappeared under the use of arsenic.

Chlorosis is not such a widely different disease. The fact that some cases of chlorosis seem to pass into pernicious anemia raises the thought that the two disorders do not start from widely different bases. Chlorosis is sometimes present in the early history of a case that later passes over into pernicious anemia. It is true, there is a reversal of the relations of the hemoglobin and the corpuscles. Ignorance still prevails as to the nature of the hemolytic substance. The modern studies in the direction of toxic substances may help somewhat, but there is considerable yet to be learned. Dr. Tyson was not hopeful of anything very encouraging from the use of bone-marrow as a remedy. Clinical experience substantiates this view.

The President related a marked case of pernicious anemia with a reduction in corpuscles to 800,000, and a hemoglobin-percentage of about twenty per cent. Most remarkable improvement occurred in a very short time under the use of progressively increasing doses of arsenic, light systematic massage and oxygen. The patient improved to such an extent that the arsenic was temporarily suspended and bone-marrow was administered. The preparation used consisted of two parts of bone-marrow and one part of glycerin, rubbed up to a paste in a mortar, and this was spread upon bread. In the treatment of profound anemia, the clinician should not be bound to any one method of treatment. Among other things, a carefully regulated diet is most important. Measures to increase quantitatively the amount of blood in circulation, such as the use of large quantities of food and of the normal salt-solution, hypodermically, together with light systematic massage and regulated exercise, are very important in the management of these cases.

Dr. D. D. Stewart referred to two cases that were treated with bone-marrow—one with

success and the other without any result. One case was considered as moribund. Every method of treatment had been tried, and the patient was very low. Bone-marrow was given in quantities as large as he could take, with extraordinary result. The man was soon up and about and assisted in some of the duties of the ward. He continued in that condition until about six weeks afterwards, when he left the hospital. The other case did not improve at all.

Dr. J. P. Crozer Griffith said that in the investigation by Dr. Burr and himself, a few years ago, with regard to the iron in pernicious anemia, an attempt was made to determine whether the increase in the red marrow was the cause or the effect of the disease. Statements had been made by two European investigators that bone-marrow could be made red by bleeding, but had never been confirmed as far as could be discovered. Dr. Burr carried out experiments on dogs in an endeavor to prove this. He succeeded, by bleeding a dog profusely, almost to the point of death, in producing red bone-marrow in the long bones. This was proof that the production of the red bone-marrow was the result of the anemia and not the cause of it. The bone-marrow was red merely because it had become active. The statement that during the use of arsenic the hemoglobin increased more than the corpuscles, seems to be a proof that the hemolysis was still going on. The arsenic was curing the patient by stimulation of the corpuscle-producing organs, but in no way checked the blood-destruction and the consequent excess of hemoglobin over red-blood cells.

Dr. Stengel said, in conclusion, that extravagant claims had been made for bone-marrow as a remedy by some, while, on the contrary, it had been regarded as useless by others. The number of cases is so small that a decided opinion can hardly be formed; but, apparently, the agent is of doubtful value. In one of the cases reported it acted marvelously well to all appearances; in another it was without effect. Arsenic seems to stimulate hemogenesis and it is necessary to continue its use for a long time—perhaps for years. Other remedies or methods of treatment may be necessary, and it is to some extent a matter of regret that arsenic acts so well in most instances. There are cases, undoubtedly, in which this remedy alone is useless and in which a knowledge of all other plans of treatment is required.

AUTO-INTOXICATION PRODUCING EPILEPTIFORM CONVULSIONS; HYSTERICAL SPASM IN THE MALE; INTESTINAL OBSTRUCTION—APPENDICITIS—PERITONITIS—OPERATION—RECOVERY; RELAPSE IN TYPHOID.

H. A. HARE, M.D.

[Read April 22, 1896.]

I desire to report these cases because of their peculiar interest both from an etiologic and therapeutic standpoint. The first is that of a clergyman, aged fifty, for many years a resident of Texas, in which portion of the country he suffered from a severe attack of yellow fever some years ago, and since which he has never been "the same man," although he has worked as hard and has been as active as before. He is well nourished and apparently in good physical condition, and has been in the habit for a number of years of taking a large amount of daily exercise, often walking from seven to ten miles every day for weeks at a time, with the hope that he might be able to overcome the condition about to be described. Having a tendency to chronic constipation, which usually requires a careful laxative diet and active laxative medicines to overcome it, the man has hoped that exercise would give him a certain amount of relief. As often as once a week, or even oftener, he suffers from fulness in the head and some dizziness, with a feeling of general malaise and wretchedness, and during this time it requires the greatest force of his will to make any effort, mental or physical. At less frequent intervals, varying from a monthto six months or more, he has been seized with much more violent attacks, in which for several days the symptoms just described persist; while he wakes early in the morning, rises from his bed in a semi-delirious condition, and, if interfered with, is likely to become somewhat violent. In a short time the man passes into a typical epileptiform convulsion, followed by deep sleep and a restoration of consciousness, entirely similar to that seen in a person emerging from a true epileptic paroxysm. There is no history of any specific trouble, of any injury, of sunstroke or hereditary tendency, although there is a history of marked nervous debility due to overwork at various times during the past twenty years.

Carefully directed treatment for the purpose of overcoming the nervous exhaustion and of improving the digestion effected little if any result, and the fact that large doses of calomel had on a few occasions seemed to produce temporary improvement suggested to my mind the possibility that the case might be one of auto-intoxication, somewhat similar to those described by Brieger and by some French investigators. Acting on this principle the patient was directed to take a diet as free from fats as possible, and to produce every day by means of a copious draft of Hunyadi water, taken before breakfast, a semi-formed stool. He was also given at the same time a pill composed of extract of chiretta, two grains; leptandrin, half a grain; podophyllin, one-fifth grain; euonymin, half

a grain; beechwood creosote, half a grain; taken three times a day after meals. During a period of three months he has not only had no return of his epileptiform manifestations, but he has been entirely free from the symptoms of mental and bodily torpor already mentioned; and it would seem evident that by the regulation of the diet and the stimulation of the gastro-duodenal glands and the glands opening into the duodenum, as well as by the use of the purgative, certain processes that resulted in the development of animal alkaloids in the intestines were set aside.

The second case is that of a man, aged nineteen, of Irish birth, who was confined to bed because of violent pain in the epigastrium radiating toward the right kidney. He was generally free from the pain during the day until late in the afternoon or in the evening, when it would come on with what he described as frightful intensity, so as to be absolutely unbearable. His tongue was heavily furred and his digestion evidently markedly impaired. He did not, however, possess the appearance of a person who suffered severe pain, nor was there any evidence of impaired nutrition or grave disease. On attempting to examine the abdomen, it was found that both rectus muscles were in a condition of rigid spasm, and when they were touched they developed, particularly the one on the left side, rhythmic contraction, which conveyed to the hand an impulse very similar to that produced on deep palpation of the aorta in a person with a thin abdominal wall. The greater the pressure the greater was the response to the spasm. The time of the spasmodic contractions was about sixty per minute. On anesthetizing the patient the spasms ceased entirely, long before ordinary muscular relaxation was produced, and deep abdominal palpation failed to reveal anything abnormal in the belly. Largely for the purpose of producing a mental effect and also with the hope of making an impression upon the condition of the stomach, lavage was instituted. Preliminary examination of the gastric contents revealed a condition of chronic gastric catarrh, with advanced fermentation of the stomach-contents and excessive mucus. Persistence in the use of lavage and antiseptic solutions and the regulation of the diet caused entire removal of all the symptoms.

The third case is that of a boy, aged nineteen, who was taken ill on a Monday night with violent pain in the belly, which lasted continuously until I saw him on Tuesday evening at nine o'clock. On examining the belly all the physical signs of a general peritonitis were present. The abdominal wall was hard and knotted, but scaphoid and tender on pressure. The pulse was 154, the respirations fifty-six. The pain was localized in the neighborhood of the diaphragm; there was no increase in tenderness in the right iliac fossa, nor, indeed, could any excessively tender spot be found anywhere in the belly. The bowels were obstinately confined; the urine was secreted in fair quantity, but there was difficulty in passing it, owing to the pain that was produced. Careful rectal examination showed possible tenderness in the right iliac fossa, but this symptom was not sufficiently marked to make a diagnosis of appendicitis justifiable. There was no history of a previous attack of appendicitis, but the patient stated that six months before he had suffered with violent pain in his side, which his physician had called "rheumatism."

As it was evident from the boy's condition that under medicinal treatment death must shortly result, and that quickly, a surgical operation was strongly advised after consultation with Dr. W. W. Keen. The patient was at once removed to Jefferson Medical College Hospital and at midnight was operated upon by Dr. J. Chalmers Da Costa. Examination of his heart prior to the operation revealed an exceptionally loud aortic obstructive murmur. As there were no symptoms indicating localized trouble in any portion of the peritoneum, a median incision was made, and almost immediately it was found that the ileum was obstructed in consequence of the intestine having fallen over an old band formed by some previous inflammatory process, and it was supposed that this obstruction was

the cause of the apparently universal peritonitis. For the sake of leaving no stone unturned, in view of the obscure character of the case, the appendix was drawn into the opening in the abdomen and was found to be intensely inflamed and bent in coils which were bound together by old and new lymph. The canal in the appendix was also practically obliterated near its junction with the colon. This portion of the bowel contained no foreign body or fecal matter, but it contained some mucus and the mucous membrane was so thickened and hardened as to be almost cartilaginous. The appendix was removed. The operation occupied an hour and a quarter. The patient was put to bed in fully as good a condition as before the operation, the abdominal cavity having been in the meantime thoroughly washed out with hot saline solution. The case progressed favorably until the end of thirty-six hours, when it was thought wise to remove the drainage-tube. Almost immediately after its removal the patient expressed himself as feeling much worse, and began vomiting the peculiar greenish matter characteristic of some cases of peritonitis. The ejecta speedily became stercoraceous in character and in odor, and it was believed that a fatal result impended. The wound was opened and deeply packed with gauze in order to re-establish drainage, and immediately upon this being done the patient markedly improved. He was given by the mouth small quantities of cracked ice over which had been poured white of egg and some brandy for the purpose of relieving the pain and tympanites; and to quiet the nervous restlessness and stimulate the vaso-motor system he was given a rectal injection of starch-water containing fluid extract of hyoscyamus, five drops twice a day. Following this he passed through a long period of convalescence lasting weeks, and during which there were frequent attacks of violent pain in the belly, with vomiting. At one time the vomiting occurred daily, but ceased when the morphin was withdrawn and the bromids and chloral substituted therefor as hypnotics. At the time of the attacks of pain and vomiting, which occurred after the morphin had been stopped, the surface of the body would become icy cold and covered with sweat, the temperature subnormal, and active treatment was required to prevent fatal collapse, such as the application of heat and the rectal injection of strong, black coffee.

The surgical complication of the case consisted in a gaping of the wound in consequence of the removal of the stitches and the constant packing with gauze. On one occasion active hemorrhage took place from the denuded surface of a knuckle of intestine which was constantly exposed in the wound. By means of adhesive strips and an abdominal binder and rigid antisepsis the edges of the wound were approximated, became healed, and with the aid of an abdominal belt the patient left the hospital weeks after admission, having gained considerably in weight during the last few weeks of his sojourn.

The entire course of this case is exceedingly interesting, but the points of peculiar interest are the development of general peritonitis from intestinal obstruction; the association therewith of acute appendicitis; the fact that had medicinal measures been relied upon in the absence of distinct indications for an operation, death would certainly have occurred; and, finally, that recovery should have taken place after the characteristic vomiting had been present in a person who already had to contend with marked aortic obstruction. The maintainance of absolute asepsis, so that the edges of the wound at no time became infected, notwithstanding that it had to be dressed daily week after week, speaks well for the care of the attendants and had much to do with the ultimate recovery of the patient.

A man, aged twenty-three, was taken ill with typhoid fever on February 1, 1895. He passed through the characteristic period of four weeks without any symptoms of particular interest, save that he had repeated hemorrhages from the bowel, varying in quantity from a few drams to five ounces. His temperature having been normal for three days, much against my better judgment and because of his constant

pleading, I added to his milk-diet a small quantity of broth containing rice in considerable amount. Three hours after taking this meal the temperature rapidly mounted from normal to 104°, and a typical relapse, not a recrudescence, occurred. The tongue again became dry and characteristically coated, the typical typhoid eruption appeared on the abdomen and chest, the diarrhea returned, and the characteristic morning fall and evening rise of temperature, with gradual progression during three weeks back to the normal, followed.

This case is of interest as indicating that a true relapse may follow the ingestion of food, although as a rule when food is given too early, a temporary return of the fever, which is of an irritative type, while it may alarm the physician, does not indicate a true second attack of the disease.

DISCUSSION.

Dr. Joseph Price dwelt upon the importance of the central incision in cases like the one of appendicitis reported. It is also a matter of moment to free all adhesions in order to prevent the possible recurrence of symptoms of intestinal obstruction. The success of surgical treatment depends upon the relief of sepsis, and to this end the open treatment is most advantageous.

Dr. Hare added that it was common in his experience with typhoid fever for either recrudescences or relapses to follow the too early ingestion of solid or semi-solid food. It would seem as though the change from a milk-diet to other form of food favored in some way renewed activity on the part of the typhoid and perhaps also other intestinal bacteria.

SOME THERAPEUTIC USES OF GUAIACOL.

HORACE G. McCORMICK, M.D.

[Read April 22, 1896.]

I desire to give my experience, gleaned from bedside observation, of that little-written-about drug, guaiacol.

Its use as a local application for the reduction of temperature was first suggested to me by Dr. J. Solis-Cohen, some two years ago, in a case of tuberculosis which he saw in consultation with me. Its action in this case was so prompt and effective that I pushed the investigation in other diseases, with results herewith summarized.

In September, 1894, when I entered upon my term of service in the Williamsport Hospital, I commenced the use of guaiacol as a local application for the reduction of temperature. In typhoid fever, since then, I have had it applied 864 times—778 times in the hospital, and 86 times in private practice. These applications were made to forty-three different persons, about equally divided between males and females, with wide variation of ages. The greatest number of times it was applied to any single person was 78, and the least number of times once. The largest dose was twenty-five drops and the smallest two drops. The greatest reduction of temperature was from 106.8° to 101°, in two hours, by the application of five drops, with a corresponding reduction of the pulse-rate from 136 to 110 per minute, the respirations falling from 36 per minute, to 28. This patient, however, showed very great susceptibility to the drug, as the application of two drops reduced the temperature from 103° to 100.4° in one and one half-hours. A number of special reports were prepared for me by the nurses of some of the cases in which guaiacol was applied, which may be useful in showing its effect not only upon the temperature but also upon the pulse:

Carrie Horton:

Five drops of guaiacol applied.	Pulse.	Temp.
9.45 P.M.	144	107°
10.20 P.M.	132	104.2°
10.50 P.M.	132	102.2°
Five drops applied.	**Pulse.**	**Temp.**
12.45 P.M.	130	104°
1.30 P.M.	120	100.6°
Five drops applied.	**Pulse.**	**Temp.**
5 A.M.	118	104°
5.30 A.M.	108	102.4°
Five drops applied.	**Pulse.**	**Temp.**
1.25 P.M.	135	104°
2.20 P.M.	130	101.8°

Ada Saxton:

Twenty drops applied.	Pulse.	Temp.
11 A.M.	126	104°
12.30 P.M.	112	101°
3 P.M.	110	100°
4 P.M.	122	99°

Annie Witzman:

Ten drops applied.	Pulse.	Temp.
10 P.M.	130	104.4°
11 P.M.	108	100.6°
12 midnight	105	98.2°
12.30 A.M., slight chill.		
Fifteen drops applied.	**Pulse.**	**Temp.**
12 M.	126	105.2°
1 P.M.	120	101.4°
2 P.M.	106	100.8°
Fifteen drops applied.	**Pulse.**	**Temp.**
10.40 P.M.	156	107°
11.15 P.M.	134	104°
12 midnight	116	102.4°
12.45 A.M.	108	99.8°

It will be noticed that within thirty minutes after the application there was a fall in the temperature, and in most cases a corresponding reduction of the number of heart-pulsations per minute. It is generally asserted that guaiacol is a depressant, and, for this reason, a dangerous remedy in diseases in which the circulation is likely to suffer from long-continued fever. This assertion has not been borne out by my experience. On the contrary, I have seen at different times, with a high temperature, a pulse so rapid and weak that it could not be accurately counted after the application of guaiacol distinctly lessened in frequency and strengthened in force. A weak and rapid pulse is to me no contra-indication for the use of the drug.

In the case of Annie Witzman the reduction of temperature is well illustrated; seventy-eight applications were made. It will be noticed that on November 11th, at 10.40 P.M., the temperature reached 107° and the pulse was 156. Fifteen drops of guaiacol were then applied, and at 12.45 P.M. the temperature was reduced to 99.8° and the pulse was 108. The table does not show this. The ice-pack was used in this case, being kept up for twelve consecutive hours; yet I was forced to abandon this and return to the guaiacol in order to reduce the temperature. This case was one of the most persistent cases of high temperature I have ever seen. The patient made a good recovery.

The effect of guaiacol lasts from three to four hours; the more often it is applied, the greater the effect. When I first commenced the use of this drug I found that the sudden reduction of temperature caused chilling in a number of cases, but after I became more accustomed to its use chills rarely occurred. If it can be avoided, the temperature should not be reduced below 100°; and this is a matter which can easily be regulated after the applications have been made a few times, care being taken to commence with a small dose—say from ten to fifteen drops—this gradually being increased if necessary until the temperature is reduced.

It has been suggested that by this rapid reduction of temperature there is great danger of producing congestion of some of the internal organs of the body. I have not seen a single unfavorable symptom (except an occasional chill) following its use. I had one case of pneumonia as a complication, but this did not develop until six days after the last application of guaiacol, which was in no way responsible for it.

The point selected for the application was the right iliac region. This was thoroughly cleansed with soap and water, and after the part was thoroughly dried (it is important that the surface should be entirely free from moisture, for guaiacol, being of an oily nature, will not be absorbed if there is the least moisture present) the guaiacol was slowly dropped upon the surface and thoroughly rubbed in with the hand for from ten to fifteen minutes. The part was then covered with oiled silk or waxed paper. The only preparation used was Merck's, and it rarely failed to produce the desired result. Any other point would probably do as well for the application, but I selected this because it was as near the seat of the trouble as I could possibly get, and could be easily reached and covered with the oiled silk, without in any way disturbing the patient. In only three cases was any local irritation produced, and I was forced to move to the left side to make my applications.

There were no unpleasant symptoms accompanying its use, and no complaint was made by any of the patients. The disagreeable odor that has been described by some as being objectionable was referred to by only one of my patients, and then only on the first application. Sweating is nearly always produced, corresponding somewhat to the greater or less reduction of temperature.

One very important fact was observed in the use of this drug for the reduction of temperature in a case of pyemia. The case was seen in consultation with Dr. Detwiler, and was found with a temperature of 107°. Twenty drops of guaiacol produced no effect. The application was repeated in forty minutes, and yet no effect on the temperature was produced. Fifty minutes later

another application (this time forty drops) was made, with the same result as before. The applications were continued until the expiration of three hours, when 100 drops had been applied; yet during this time the temperature had not varied over half a degree from 107°. The pulse, which was weak and rapid when the first application was made, at the end of three hours had lessened in the number of beats per minute and increased in volume. There was no sweating in this case. I have not had an opportunity of trying guaiacol in any other case of pyemia, but from my experience in this one I am led to believe that it has no value.

It may be urged that the fever of typhoid is not the disease, and does not call for treatment. That it is the disease no one will pretend to assert; but that it is an important symptom, and when it rises beyond a certain point calls for something to reduce it, or keep it in abeyance, every physician who has had much to do with this disease can bear testimony.

When the system of baths was first introduced, the only object was the reduction of temperature, but those using them found that their patients did so well under this form of treatment that they took to philosophizing upon the subject, and undertook to prove that the cold bath did something else not exactly admitting of explanation, by modifying the disease, lessening the fever, and making the case a less formidable one. That this effect is produced I am willing to concede, but that the baths do more I believe is open to serious question, and further proof will be required to place it beyond controversy in the profession.

Much has been written by the advocates of the system of the delight and pleasure their patients have experienced in being put in a cold bath. I believe these gentlemen have never tried it upon themselves. I have no reason to believe that the people of Philadelphia or Baltimore are any less susceptible to cold than those in the interior of Pennsylvania. I have used the cold bath many times, and, with the exception of a colored boy whom I treated in this way some time ago, I have not been able to make my patients believe by any argument of mine that it was an enjoyable pastime. I thoroughly believe in it as an effective form of treatment, but it is not pleasant for the patient; it is difficult to carry out in detail; it involves great expense, and, in private practice, is not a practical form of treatment. In spite of the inventon of portable bath-tubs, the method still lacks that practicability that is demanded by the general practitioner.

Guaiacol when given internally does not markedly reduce the temperature. I have given as high as ten drops in a single dose to a patient without materially affecting the pyrexia, while the same amount thoroughly applied to the skin would reduce the temperature in a marked degree. I have also applied the remedy to those in health in sufficient doses to reduce an abnormal temperature, without any effect upon either the temperature or the pulse.

The natural question to ask would be: If this drug possesses the power as here asserted, what is its mode of action? How does it reduce temperature? What is the physiologic action? Now, in order that there may be no controversy, with me at least, upon this subject, I candidly assure you that I do not know. I know the effect, but cannot explain how it is brought about.

While I have a high regard for guaiacol as a local antithermic, none of this respect is lost when I come to use it internally. As an intestinal antiseptic I believe it has no superior. I have used it in fifty-six cases of typhoid fever, with the best results. The best evidence of its virtue in these cases is the fact that tympanites was either entirely abolished or was present in so slight a degree as to be of no consequence. The tongue was moist throughout the disease, and delirium if present was of a very mild form. Of the fifty-six cases thus treated, fifty-five recovered. In the one in which death took place the patient was admitted to the hospital on the fourteenth day of his sickness, and died on the nineteenth—of perforation; an autopsy was made, and it was found that the perforation had taken place about three inches above the ileo-cecal valve. When the

abdomen was opened, the unmistakable odor of guaiacol was found to be present. This was to me an important discovery, as by the death of this man was proven —what I had before been led to believe —that guaiacol passes through the alimentary canal unchanged, for the guaiacol which had been given this man was still guaiacol at the ileo-cecal valve, and its passage through the colon would not be likely to change it. The smell of guaiacol was noticed in the stools of nearly all of these patients. It has been my practice to give pure guaiacol in emulsion. This is rather a disagreeable dose, and some objected so seriously to its use that I was forced to give it in capsules; when this was not expedient, then I gave guaiacol carbonate dry upon the tongue in $2\frac{1}{2}$ grain doses. I have never found this to disagree with the stomach, and patients do not offer any objection to taking it.

In summing up this question I feel convinced of the following facts:

That guaiacol when applied locally in typhoid fever is certain to reduce the temperature.

That, with the care that a physician should always use in the administration of drugs, it is absolutely safe.

That chills will not occur if the temperature is not reduced below 100°.

That no deleterious effects are produced upon any organs of the body by its use.

That it is easy to apply and can be used by any one competent to nurse a typhoid-fever patient.

That there are no depressing effects following the use of the drug, if used intelligently.

That by its continued use the dose can be gradually lessened.

That it is far superior to the cold bath, in that it can be used by one person. No appliances are necessary for its use that are not obtainable in any house. It is much more pleasant to the patient, and carries with it no horror to the family. Argument and persuasion with the family and friends are not made necessary. It is fully as effective as the cold bath. Patients are not subjected to the danger of moving. There is no complication such as hemorrhage, etc., that is a contra-indication to its use.

That when given internally it is one of the best intestinal antiseptics known, if not the best. By its use in typhoid fever the dry tongue and tympanites are abolished, digestion and assimilation rendered more perfect, and the probabilities of hemorrhage reduced to almost *nil*.

DISCUSSION.

Dr. J. V. Shoemaker stated that he had employed guaiacol especially in the treatment of such diseases of the skin in which formerly it had been the custom to employ creosote and carbolic acid—e. g., superficial epitheliomata, lupus vulgaris and old ulcers. The remedy has also been employed topically with success in the treatment of orchitis and epididymitis.

Dr. H. A. Hare pointed out that guaiacol has pretty much the same powers and dangers as the coal-tar antipyretics, antipyrin, acetanilid, phenacetin, etc. It is, however, not so useful as the cold bath intelligently applied. It must be remembered that fever has some protective influence and may be viewed as the reaction of the organism to the invasion of the disease-process. The cold bath does more than reduce temperature. It has, besides, a distinctly vitalizing influence, stimulating in general the functional activities of the organism. Dr. Hare criticised the routine employment of the cold bath and maintained that this procedure is not to be pursued in every case, but only when other measures prove insufficient. The question is essentially one of dosage, and it is necessary to adapt the treatment to the disease as seen in individual cases.

Dr. J. P. C. Griffith said that he had used guaiacol externally in a number of cases of typhoid fever, but had given it up because of his inability to control its action. It had seemed to him difficult, if at all possible, to determine in advance how many drops of the remedy will be necessary to effect a given reduction in temperature, the dose affecting one patient not at all inducing a great fall in temperature in another.

Dr. J. M. Anders related that in an ex-

tensive experience with guaiacol he had twice observed its application to be followed by a rise of temperature to a point higher than the previous maximum temperature. He had therefore abandoned its use in cases of typhoid fever, and had confined its employment to cases of subfebrile and afebrile disorder. Thus the drug had yielded good results in the treatment of myalgias and neuralgic pains. For this purpose it was mixed—as Ferrard first suggested—with an equal part of glycerin and painted upon the affected surface. He had found that its hypodermic use proved even more effective than the topical application, a minim or two of guaiacol being mixed with ten minims of chloroform, and the dose repeated as needed.

Dr. Frank Woodbury pointed out that the action of guaiacol is probably to be attributed to its absorption into the blood and its influence on the thermic centers. Guaiacol has also proved useful in the treatment of pulmonary tuberculosis. It constitutes about sixty per cent. of creosote, which is the best single agent at present known in the treatment of tuberculosis. Carbolic acid and guaiacol do not act alike; the former is far the more toxic. On account of its disagreeable taste, guaiacol has been, in his own practice, largely superseded for internal administration by guaiacol carbonate. Under this plan of treatment it has been observed that hectic is reduced and retrograde changes effected in the morbid process in the lungs.

Dr. S. Solis-Cohen stated that he had had an opportunity of seeing some of Dr. McCormick's cases and had come to the conclusion that the patients had passed through a dangerous disease as safely and as comfortably as after any other mode of treatment. Nevertheless, he was unable to agree that the drug should supersede the cold bath. Briefly stated, it is an excellent means of treatment in suitable cases. Guaiacol internally is, like salol, an excellent antiseptic, and is capable of controlling the fetor of the stools. For this purpose the carbonate and the salicylate may also be used. It is a fair question whether the reduction of the temperature in cases of pulmonary tuberculosis by means of applications of guaiacol really does good and whether the patient is better off for it. The reduction is not permanent and the applications, to be effective, must be repeated frequently. The matter of personal idiosyncrasy must be taken into consideration, some individuals reacting to a moderate dose, while in others a large dose induces but a slight effect. Applications of guaiacol seem to prevent the growth of the diphtheria-bacillus in the throat. Dr. Cohen referred to two epidemics of diphtheria at the Pennsylvania Training School for Feeble-Minded Children at Elwyn, in which the outbreak was brought to a close by the topical application to the throats of the healthy children of guaiacol diluted with 50 per cent. of sterilized olive-oil, with the addition of ten per cent. of menthol.

Dr. A. B. Kirkpatrick referred to a half-dozen or more cases of typhoid fever in which he had administered guaiacol carbonate internally, with the result of having the tongue clear and the appetite and digestion improve, while the temperature declined gradually from day to day, becoming normal in several cases within two weeks or less. The drug had also proved useful in the treatment of entero-colitis as a very safe and efficient intestinal antiseptic.

Dr. McCormick, in conclusion, replied that he had not observed any increased elevation of temperature after topical applications of guaiacol. The drug is, of course, not without its dangers, as all active medicaments are, but in the hands of the intelligent physician these are minimal. Whether the application shall be made or not must be determined in every individual case from the symptoms and general condition. Dr. McCormick repeated that while the fever is not the disease, it must at times be treated, whether guaiacol or the cold bath or other means are used for the purpose.

RECTO-VAGINAL ANASTOMOSIS.

JOSEPH PRICE, A.M., M.D.

[Read April 22, 1896.]

In malignancy of the uterus—in cervix, fundus, or both, we occasionally have invasion of the bowel. I have many times extirpated the uterus and found the invasion about the left-broad ligament, rectum or sigmoid. A few of these cases have later required inguinal colotomy. Thrice in cases managed by the non-operative method the fundus uteri and sigmoid have fused, the contents of the bowel passing through the uterus and vagina for some weeks or months before death. It occurred to me that extirpation of the malignant uterus and bowel,with an anastomosis of vagina and large bowel, would be a much safer, cleaner, and more radical procedure, if the cases could be operated on before general invasion. This form of operation is the only one by which the difficulties encountered in these cases can be overcome with any promise of favorable result.

In the case I report the pathologic conditions were peculiar and mixed. The woman was about forty years of age, and had given birth to seven or more children. She was small, feeble, and exhausted from disease and prolonged suffering. Vaginal examination showed everything fixed and immovable—physical signs of pus on the right, and cystic disease on the left, with a large uterus studded with large fibroids. In the operation I found an old suppurating tube and ovary on the right side; on the left a malignant tumor about the size of a child's head, firmly fixed to the posterior wall of the uterus and anterior wall of the rectum.

After freeing the diseased tube and ovary of the right side, the enucleation of the malignant tumor on the left was found very difficult; it had invaded the bowel and uterus. I determined to remove the uterus, with the malignant section of the bowel. The hysterectomy was done by the anterior method, incising anteriorly and retracting the bladder—opening the anterior vesico-vaginal pouch and tying backwards—doing the operation from in front backward—not from behind forward. Two or three ligatures right and left were sufficient. The danger of wounding the ureters is about nil, as they are pushed well off the cervix. This anterior method of extirpating the uterus gives a better field for rapid work than any other method of extirpation in which ligatures are used. Vaginal incision fore and aft the cervix, pushing the incised vagina well up makes the method simple and rapid. After completing the hysterectomy I amputated the bowel in healthy structures above and below and stitched the proximal extremity of the bowel in the upper extremity of the vagina, the size and mobility of both tubes favoring easy and rapid work. The patient made a ready recovery, with daily evacuation after the fourth day. After evacuation of the bowels and a vaginal douche, at no time during the day was there a perceptible fecal odor or evidence of discharging of feces.

In colotomy we commonly have a bad atmosphere to live in—patients sometimes complain bitterly of the disgusting opening—and further, the malig-

nant bowel remains after such a procedure. Again it is only the upper rectum and sigmoid portion of the bowel that will permit of such a procedure. In two cases the operation has worked successfully, very much better than an anastomosis of the large bowel after removing a segment. Both cases recovered after the anastomosis, and one recovered with the button-method. The other case died on the sixth or seventh day when I considered her in a fair way to a good recovery. Age may have had much to do with her death. She was up in the sixties.

The operation may be tersely summarized as follows: Complete extirpation of uterus by the anterior method from the bladder back, and extirpation of malignant zone of bowel, and turning of bowel into the upper extremity of vagina.

Another operation to which I would refer, and one which I have not seen mentioned in our literature or noted in the reported work of other men, is that of extirpating the diseased appendix, cutting it out of the head of the cecum and closing the incised opening. All the patients upon whom I performed this operation recovered. The operations were done in the very class of cases now so generally and freely discussed in relation to removal of the appendix. The questions involved, the surgical problems, are so very vital in their interest and bearing that I will not attempt, in this paper, to reconcile controverted points. I will simply report two typical cases, illustrative of my opinion and practice. Fortunately, we have reached the end of all dispute as to the propriety of seeking and stitching up shot and incised wounds of abdominal viscera. All such wounds are now trimmed and sutured after free discharge of their contents. The abdomen is opened, wounds repaired, vessels tied, the peritoneal cavity cleansed, and sometimes drained, and speedy recovery follows. In view of the facts in this connection, the uniformly favorable results secured, the inquiry is a natural and forcible one: Why should the profession bother about a large variety of methods in dealing with a dirty, disorganized appendix, inverting it and in a variety of ways doing that which should not be done, and leaving that which in the majority of instances works mischief? Why leave behind seeds to produce a future growth of trouble and suffering. Certainly it would be better to leave behind, as nearly as possible for our surgery to accomplish, wholesome conditions. Simply cutting out the dirty sinus, useless and an element of danger from its very disorganized state, gives just as good results when the small opening is closed as can be obtained at other points, and thus are avoided post-operative sequelæ, fistulæ and lurking dirty ligatures, which always delay perfect recovery. I am aware that there are cases in which the extirpation-method will not answer. The cecum is dirty and disorganized, conditions not favoring the use of sutures. But in this group of cases one can stitch the head of the cecum as safely as the small bowel can be stitched, when cheesy, disorganized, and adherent, to a large pus-tube or an ovarian abscess, and as safely as the bowel can be sutured for perforating ulcer in typhoid fever. Surgeons are prone to allude to infection by the extirpation-method. The reply is ready at hand, strong and difficult to answer. The critic's method is itself the best answer, that of transfixing a pus suture through the dirty sinus and permitting it to be folded in clean tissues. The inversion-method does not always result in the diseased stump going into the cecum. A number of the cases thus treated are followed by fecal fistulæ when the cecum is apparently healthy and there is no abscess about it. I see by recent articles that the profession still dreads peritonitis or general sepsis. I am glad such apprehensions only exist with general surgeons and not with the abdominal or pelvic surgeon. Our prolonged experience in suppurative forms of tubal and ovarian disease, with the disease extending from cecum to sigmoid, gives every assurance of our ability to manage such cases without infection or sepsis. It is very well known that the peritoneal cavity and viscera have a thousand times been bathed and manip-

ulated in pus, charged with every variety of organism, without the slightest evidence of sepsis. And when sepsis antedated the operation, the removal of the disease and septic fluids resulted in its prompt arrest. Such results have been obtained by a number of operators. The man who dogmatically states that a case of general septic or purulent peritonitis has never been saved should limit his remarks to his own experience. He is not competent to speak for others. To illustrate how others can save patients suffering from appendicitis by the extirpation-method, I will quote my friend, Dr. Torney, U. S. A., West Point:

"I have had an appendicitis operation since I saw you, in which I followed your methods as far as I was able; removed the entire appendix and sewed up the incision in the bowel wall. The patient is well."

DISCUSSION.

Dr. George Erety Shoemaker said that while the cutting out of the end of the appendix from the cecum may be useful, if that part of the organ is cheesy or is sloughing this is not usually necessary. Almost always the disease is found not to have seriously involved the end of the appendix nearest the cecum, as that part has been well drained. Ligation also secures quick and thorough control of the blood-supply, while invagination of the stump yields perfect results without any danger of a gush of feces, especially when it is necessary to operate without securing perfect cleansing and collapse of the bowel by previous purgation.

The method of tying off the broad ligaments from in front in doing pan-hysterectomy from above seems scarcely to differ from the methods commonly employed and described in journals and books. After dissecting down the bladder and opening the vagina, the operator will naturally tie off in the most accessible direction. Dr. Shoemaker referred to a case in which, under special necessity of avoiding a misplaced ureter, he tied around from the front, with no thought of doing anything but an adaptation of a well-known operation.

Dr. M. Price reported a case of appendicitis in a young man, twenty-two years old, on whom he operated thirty-six hours after the first symptoms had appeared. The appendix was covered with inflammatory lymph, and the bowel was everywhere adherent. The peritoneum covering the appendix was amputated down to the indurated and thickened appendix. This was pushed back to the head of the colon, and the hard and indurated portion of the appendix cut out of the head of the colon. The peritoneum was pushed into the opening, and sutured with Lembert sutures The vessels were tied in the mesentery. Thorough separation of adherent bowel and irrigation with gauze and rubber drain were effected, and recovery ensued without bad symptoms.

NECESSITY FOR THE STATE TO DEMAND THE ADOPTION BY THE COLLEGES OF A MORE UNIFORM STANDARD OF EDUCATIONAL REQUIREMENT FOR THE DEGREE OF DOCTOR OF MEDICINE.

SILAS UPDEGROVE, M.D.

[Read May 13, 1896.]

The result of the recent application to the General Assembly for the passage of an Act to create a Board of Examiners gave emphasis to the importance of a demand for the adoption by the colleges of a more uniform standard of educational requirement for the degree of doctor of medicine.

The application resulted, as we all know, in establishing three boards instead of one for the examination of candidates for a license to practise medicine, not with the intention, however, of having the candidate examined by each of these boards, but that certain candidates might have a board suited to the standard of educational requirement of the college from which the degree was received.

The creation of different boards served to accentuate the fact that the possession of a degree could not be accepted as evidence of a uniform standard of qualification, but only as evidence of a standard of qualification in accordance with the standard of educational requirements of the college from which the degree was received, and that an examination must be adapted to these standards.

In attempting to justify the creation of more than one board of examiners, the absurd claim is made that these alleged standards of certain colleges are each to represent an educational requirement according to a formula of treatment or alleged method of cure, and the creation of separate boards was asked for, in order that the examination might be adapted to the standard of qualifications of the candidate whose degree was received from colleges having a method of cure as the basis of the educational requirement.

These standards, according to their alleged methods of cure, will, if possible, be discussed in such form as to enable the public to place the true estimate on their value as the basis of an educational requirement for the degree of doctor of medicine.

The public can readily understand that the maladies of mankind may be divided into two classes, viz: First, the curable, and second, the incurable. No one perhaps should dispute this classification, nor should any one possibly assert that he has a formula of cure for the diseases of the class called incurable. One class then is disposed of without a question as to the formula of treatment or method of cure.

The class called curable is so named, perhaps, because some of the diseases of the class get well under treatment. Some get well, however, without treatment; and yet other cases do not get well under treatment, no matter what method may be employed.

In its application to the treatment of this class a formula of cure is placed very much in the condition which ap-

plies to a formula of cure for the incurable class, viz: That no formula is known for the cure, either individually or collectively, of the diseases of this class.

To illustrate, we may take diphtheria as a typical disease of this class, and one in which not only the results, but something of the methods, as for instance the trial of antitoxins, has become somewhat familiar to the public.

Cases of diphtheria, as we all know, get well under treatment and even without treatment, while other cases do not get well, no matter what formula or method of treatment may be employed. Yet diphtheria is a type of the diseases called curable, and whatever may be truly said of the effects of a formula upon diphtheria applies to all the diseases of the class and affords an illustration of the fact that no formula of treatment is known that will always cure diphtheria or any other disease of the class to which diphtheria belongs. In these two classes, the curable and the incurable, may be placed all the diseases that flesh is heir to, and no formula is known that can, with truth, be adopted as a method of cure for diphtheria or for any of the other diseases of the class called curable. By a reputable medical college, therefore, a formula of cure should not be adopted as the basis of the educational requirement for the degree of doctor of medicine.

In direct disregard of this truth, however, not only is the claim made that a formula of cure is adopted as the basis, at certain colleges, of the educational requirement for the degree of doctor of medicine, but the General Assembly is asked to authorize the creation of boards of examiners adapted to the standard of qualification of those whose degree has been conferred with this false pretence as the basis of the educational requirement. That a formula of cure should not be made the basis of an educational requirement for a degree is as incontrovertibly demonstrated as is the demonstration that the theory of Ptolemy should not be made the basis of the educational requirement in astronomy. If the theory of Ptolemy, which makes the earth the center of the solar system, cannot be accepted as the basis of the educational requirement of a school in astronomy, the doctrine of similars as a formula of cure cannot rationally be accepted as the basis of an educational requirement of a school in medicine. There are no technicalities here that the public cannot understand; nothing obscure in the conditions. The plain fact is, there are no so-called "schools" in medicine except those having a false pretence as the alleged educational basis. In addition, however, to the fact that these conditions are such as to be intelligible to the general public, the State, in order to have true medical education, needs and must have a recognized medical authority.

In all other departments of knowledge the State recognizes an authority in each respectively, and the department of medicine cannot in justice be made an exception. In the department of law the Supreme Court is the recognized authority. In the department of astronomy those learned in astronomy are accepted as authority; and if the State accepts the declaration of astronomers as authority that the Copernican is the true system in astronomy, why should not the medical profession of the world be accepted as an authority in declaring that a universal therapeutic formula or method of cure has not yet been scientifically established and that the adoption of an alleged formula of treatment, whether it be "Faith-Cure," "Sure Cure," "Similar Cure," or "Medical-Almanac Cure" as the basis of an educational requirement for a degree is making a farce of medical education.

What a conflict of authorities might have been averted and a mass of trouble avoided if the medical profession of the world a half-century or more ago had been recognized as a medical authority in declaring the doctrine of similars as having no value either in the treatment of disease or as the basis of an educational requirement for the degree of doctor of medicine.

By the decisions of the Supreme Court being accepted as authority on the subject, what a conflict of authorities is being averted as to the validity of certain laws, and how irrational it

would be to expect that such questions should be submitted to the public instead of to a proper legal tribunal for decision; yet medical questions are supposed to be decided by the public or by some other than medical authority. The degree of doctor of medicine must have a very uncertain value unless the State has a recognized medical authority as to what is to be accepted as medical knowledge and as to what is to be the basis of the educational requirement for the degree. If a candidate for a license to practise must have a board of examiners adapted to his method of curing diphtheria the State has not yet reached the position of being able to secure competent physicians.

In the execution of this law creating boards of examiners adapted to methods of cure, it is much to be regretted by the medical profession that a sufficient number of professional gentlemen could be found in the State to act in the capacity of examiners, when the law has no better purpose than to filch twenty-five dollars from the pockets of graduates of reputable medical colleges, in return for which these graduates are to be placed before the public as physicians on a plane with those whose so-called medical education has a basis of false pretence.

To further emphasize these absurdities in relation to the profession of medicine not only to the laws in this connection but to the rulings of courts which are alike conflicting and contradictory we may quote from the decisions of different Supreme Courts. One Supreme Court decides that the law recognizes no "schools" or systems of medicine, for the reason that there is no exact standard of practice to judge by. Another Supreme Court decides that all systems or schools are legitimate.

These decisions may be found in Hilliard's Law of Torts, 2d ed., Vol. I, page 253; also in the case of White against Carroll, 42 N. Y., page 161.

The case of White against Carroll was for calling a homeopath a quack. A verdict for $100 in favor of the plaintiff was appealed to the Superior Court and judgment affirmed. The decision in this case gave great encouragement to the homeopath on account of a well-grounded suspicion on his own part that if the term quack has the meaning usually ascribed to it he is verily a quack.

When he reflects that his alleged method of cure has a basis of false pretence his suspicions of himself have really more than theory as their basis. In reaching this decision the court of course assumed that the homeopath had a system and did not assume that his alleged system had a basis of false pretence.

In affirming the judgment of the lower court in this case, however, an Act of 1844 in N. Y. State was referred to which in effect authorized anyone without any license to practise physic and surgery, and in view of this Act of 1844 pretentious ignorance of the system, the court decided, and not the system, must be regarded as making the quack.

Whether calling a homeopath a quack might be actionable when his system should be known to have a basis of false pretence did not enter into the consideration of this case but these conditions were known to the defendant, he being a physician; but even if presented as justification might have had little weight in the condition of inconsistency in the present case and the contradictions in the rulings of different courts.

Another case is a suit for malpractice: Patten against Wiggin, 51, Maine, 594. The court ruled that, "If there are distinct and differing schools of practice as allopathic, or Old School; homeopathic; Thomsonian; hydropathic or Water-Cure, and a physician of one of these schools is called in, his treatment is to be tested by the general doctrine of his school and not by those of other schools. The jury are not to judge by determining which school, in their own view, is best." This case is so prolific in points for comment that for want of time the next and final case will be considered.

Bowman *vs.* Woods, 1 Green (Iowa) 441. The court ruled that the physician is expected to practise according to his professed and avowed system.

As yet, says the court, there is no particular system of medicine established or favored by the laws of Iowa. And as no system is upheld none is prohibited. The regular system, though advancing, is still regarded by the law with no partiality or distinguishing favor, nor is it recognized as the exclusive standard or test by which the other systems are to be adjudged. The ruling in this case furnishes a good example of the lack of knowledge of the real conditions. The system of medicine in the present state of medical knowledge, as taught by the profession of the world, is on the basis that a formula or method of cure has not been scientifically established and this is not only the system that should be favored by the laws and courts of Iowa but is the system that must be accepted as the exclusive standard or test by which the other so-called systems are to be adjudged by the laws and courts of all other states. A system on the basis of treatment by a formula is in conflict with and a contradiction of this system and must be prohibited, as they cannot both be true. The truth of the basis upon which medicine is taught by the profession of the world to-day is as incontrovertibly demonstrated, as has been already said, as is the truth of the basis of the present system of astronomy and is the standard or test by which other so-called systems are to be adjudged.

We now come to a consideration of the remedy for these anomalous and contradictory conditions. We have incorporated bodies to deal with and legal friends at once suggest the powers of the charter, vested rights, etc. But is there any real difficulty? Charters need not be revoked; colleges need not be burned nor hospitals destroyed; the State needs but to demand the adoption by the colleges of a more uniform standard of educational requirement for the degree of doctor of medicine. Our legal friends may be able to tell us whether this may not perhaps be accomplished by the simple process of injunction, commanding the authorities of the colleges to show cause why the educational standard for the degree should not be made to conform to that established by authority of the medical profession of the world.

If schools of astronomy should be attempted on the basis of the system of Ptolemy the propriety of those learned in this science adopting measures of correction should not be questioned, and on the same principle it is appropriate for the medical profession, as it is its prerogative, to have the necessary measures adopted in the line of correcting analogous conditions existing in the medical profession.

Dr. J. Solis-Cohen exhibited a

Tongue-Depressor, by means of which the Interior of the Larynx and Trachea can be inspected directly, without the aid of a Laryngoscopic Mirror.

Dr. Cohen stated that the instrument was a device of Dr. Kirstein, of Berlin, who called it an *autoscope*, but the exhibitor considered the term unfortunate, the word implying self-examination. A better name would be one suggested by Dr. S. Solis-Cohen, namely, *Ortholaryngoscope*. In reality, however, the device is a tongue-depressor. Dr. Cohen said that the difficulty in making a direct examination of the larynx is the position of the epiglottis. For this reason, we have learned to use the laryngoscopic mirror so placed in the throat as to reflect light behind the epiglottis. This ortholaryngoscope is made in such a way as to drag the epiglottis out of the line of direct inspection. In explaining its construction, the speaker said that it is a univalve guttered speculum, long enough to reach along the tongue clear to the epiglottis. The epiglottis is connected with the tongue by the glosso-epiglottic ligament. When that ligament is pressed downward and forward, exerting similar pressure on the hyoid bone, the epiglottis becomes tilted over and forward on the tongue, and is brought into a straight line. The instrument is made with a dip at the distal extremity, so as to reach down to the base of the tongue, and this dip is bifurcated so as to embrace the glosso-epiglottic ligament, and it is so nicely beveled and smoothed off that when using it for an examination, the epiglottis is given as little annoyance as is possible. The speaker described the position of the patient during the examination—seated, with trunk forward, the neck extended and the head thrown back, and with a good light above and in front, so that the examiner,

standing in front of the patient, can look directly down the throat. In certain cases, Dr. Cohen said, in which the epiglottis cannot be pulled forward in this way, a straight instrument is used, when, the epiglottis being cocainized, the instrument is brought over on top of the epiglottis, thus pressing it forward and downward upon the tongue. For convenience, an attachment in the form of a cover may be slid over the proximal end of the tongue-depressor for the purpose of avoiding trouble with the teeth and moustache. As the patient naturally is apt to close the mouth, this cap, placed upon the instrument, prevents obstruction to the rays of light. The advantage of this tongue-depressor is, the speaker said, that instead of the ordinary laryngoscopic instruments, direct instruments may be used, such for instance as the forceps and probes exhibited ; and operations can be performed much as upon any other part of the body. Dr. Cohen showed an ingenious handle for the tongue-depressor by means of which electric light, by a contrivance of hood and prism, is thrown horizontally along the tongue-depressor. This handle, adapted by Kirstein to his tongue-depressor, is Caspar's electroscope, originally devised for urethral examinations. It is an admirable search-light for surgical electric illumination generally. It is not every patient, remarked Dr. Cohen, with whom this instrument can be used. A good deal of pressure must be exerted upon the hyoid bone, but it can be used in a certain number, and no doubt, as familiarity with the device is attained, we shall be able to use it in a great many more cases than we now think possible. Dr. Cohen further remarked that this new device exposed chiefly the posterior walls of the larynx and the trachea, while the laryngoscope revealed chiefly the anterior parts, and thus one instrument supplements the other. A practical demonstration of the use of the tongue-depressor was then given by Dr. Cohen.

By invitation of the directors, DR. B. ALEXANDER RANDALL demonstrated with lantern views,

THE SURGICAL ANATOMY AND PATHOLOGY OF THE MASTOID.

CALOMEL A SPECIFIC IN DIPHTHERIA.

LAWRENCE F. FLICK, M.D.

[Read May 27, 1896.]

In the *Medical News* of April 25 I had a short article on "Calomel as a Specific in Diphtheria". When that article was written, some three months ago, I was somewhat doubtful of the correctness of my conclusions, and felt a great reluctance in announcing my views. Since then I have had opportunities for observation which leave no doubt in my mind about the specific powers of the drug in that disease. I have watched five additional cases under the treatment, four of which were in my own family, where I had excellent opportunity for studying the action of the drug.

F. S., five years old, the same person as F. S. in my former report, had been declared sterile by the Bacteriological Department, and after thorough disinfection of the house, had been going out for some weeks, when after a trip one day he was seized with a chill and high fever. Upon examination of his throat I found enlargement of the tonsils, with filling up of the follicles. Believing the child to be suffering from follicular tonsillitis I gave him phenacetin and quinin. After a few days I noticed that the follicular deposits were running together and I became suspicious of diphtheria. A bacteriologic examination proved the disease to be diphtheria. By the time the examination had been made, however, the membrane had extended over the uvula and up the posterior nares. I immediately placed the child on one-sixtieth grain of calomel every fifteen minutes. The membrane over the follicles and uvula rapidly disappeared, but the membrane in the posterior nares was apparently very little affected. Shreds of membrane could be seen hanging down, and the breathing of the child was considerably obstructed. I kept up the calomel by the mouth, and after several days of useless effort to free the posterior nares with swabs and washes, I began to insufflate the nose with a thirty-three per cent. triturate of calomel in sugar of milk. The nose began to improve at once, and in a few days all membrane had apparently disappeared. I reduced the dose of calomel by the mouth, but kept up the insufflation. After a few days the membrane reappeared over the pharynx. I increased the dose of calomel to one-fortieth grain every fifteen minutes, and resorted to pure calomel insufflation. Under this treatment all membrane disappeared in a short time, and the case proceeded to convalescence.

The other four cases appeared in my own family and were under the kind care of the family physician, Dr. William H. Parish, and were seen by Drs. Harrison Allen, Joseph McFarland, Louis Starr and William Hoch, to all of whom I owe a heavy debt of gratitude for their fraternal solicitude and kind attention. The cases, besides showing the power of calomel, are remarkable for their method of infection and unusual course.

The disease was brought into our house by a new servant whom we took in as nurse-girl. We engaged her at a reliable intelligence office, and for this reason made no inquiry into her previous history. After she had been with

us a few days my wife called my attention to the fact that she had a discharge from the nose and had large lumps on her neck. I examined her throat, but found no evidence of diphtheria, and so concluded that she had some form of influenza, and prescribed for her upon that theory. She got no better, but on the contrary grew so much worse in a few days that she went to her home. A day or two after her departure, the two youngest children, one nine months and the other three years old, began to have a slight discharge from the nose and an evening elevation of temperature. I naturally thought they had taken influenza from the girl, and put them on treatment for this disease. The nasal inflammation increased, and the slight evening rise of temperature recurred daily, but otherwise the children did not seem to be very sick. After a few days the third youngest child, about five and a half years old, began to complain of a swollen gland in the neck, back of the ear. She had no discharge from the nose and no inflammation of the throat. As she was getting a large molar tooth I concluded that the swollen gland was in some way due to the coming tooth. She was fretful, showed loss of appetite, slight disturbance of circulation, and plainly was not well, but aside from the penetrating tooth I could get no clue to the cause of her ailment.

As the nasal inflammation in the two younger children was not improving, although I had for some days been washing out their noses with hydrogen dioxid and insufflating them with boric acid, I took the older of the two to the office of Dr. Harrison Allen to seek more light on the nature of the ailment. He found the nasal passages intensely inflamed, and the post-cervical glands slightly enlarged on both sides, but did not feel justified in venturing a diagnosis. He suggested a culture to determine the cause of the inflammation. I at once sent for Dr. Joseph McFarland to inoculate tubes. So free from the suspicion of diphtheria was I at this time that when Dr. McFarland wrote me that he would come at once, and would bring some antitoxin with him in the event that it might be needed, I replied that I was absolutely certain that the disease was not diphtheria, whatever else it might be, for there was not a single clinical symptom of this disease. I had scarcely sent my letter, however, when a fourth child, about eight years old, began to complain of sore throat, and upon examination I found slight redness. As I had been attending diphtheria for some time, I began to feel uneasy lest I might have infected this child, although, up to this time, I had never associated the idea of diphtheria with the other cases. I immediately put her on one-sixtieth grain doses of calomel every fifteen minutes, and watched her throat carefully. This was in the afternoon, and by the following morning the redness had all gone from her throat and she declared herself perfectly well. I of course concluded that my fears had been unfounded and stopped the treatment. By evening, however, I received the report of Dr. McFarland, which cleared up the mystery. It was now quite evident, and subsequently proved to be so by further bacteriologic cultures, that all four children had diphtheria. I immediately placed them all on one-sixtieth grain doses of calomel every fifteen minutes, and sent for the family physician and trained nurses.

In discussing the treatment of the nasal inflammation with Dr. Harrison Allen, I had said to him that I had used almost everything I could think of locally except calomel, and that whilst I had been led to think of it by reason of the excellent results that I had obtained from its use in other diseases, notably diphtheria, I was deterred from using it in the nose by fear of injury that it might do. He assured me that I might use it without fear of doing harm. Supported with this opinion, I had already insufflated the noses of the two younger children with a triturate of calomel and sugar when I received Dr. McFarland's report, and although less than twenty-four hours had elapsed, there was already marked improvement in the local inflammation. When Dr. Parish took charge of the cases he decided to continue the calomel-treatment for the time being at least, and, with his consent, I substi-

tuted pure calomel instead of the triturate for the insufflations. Although no discharge had appeared in the nose of the third youngest child, in whom the post-cervical glands had become very large and painful on both sides, we also insufflated its nose with pure calomel.

Within twenty-four hours from the time the calomel-treatment had been begun, during which time a sixtieth of a grain, rubbed up with sugar, was given dry on the tongue every fifteen minutes, and the noses were freely insufflated with calomel, either in triturate form or pure, at intervals of three or four hours, all discharge ceased. The two younger children had, however, absorbed considerable of the diphtheric poison, and showed evidence of weak heart. Moreover, the three youngest children all had involvement of the lymphatic glands of the post-cervical region. In the third youngest child the glands were exceedingly large and painful. The danger, therefore, was from constitutional involvement and from possible extension of the disease.

The only membrane that appeared at any time in any of the cases and, therefore, the only clinical evidence of diphtheria was on the concha of the baby's left ear, which had been accidentally scratched with a pin. Over this scratch, about an inch and a quarter in length, a typical sole-leather-colored membrane formed. We thoroughly dusted over this membrane with calomel, and in twenty-four hours it dropped off, leaving a red base, which we again dusted over with calomel. In a few days the ear was entirely well.

All the children made a good recovery and were declared sterile within about ten days from the time the calomel-treatment was begun. In addition to the calomel, of which from a sixtieth to a hundredth part of a grain was given every fifteen minutes, night and day, large doses of strychnin, whisky and digitalis were given at short intervals. This constituted the entire treatment. All of the cases showed great pallor and much prostration, but these were the only symptoms that occurred throughout the duration of the disease which could indicate severity of attack. Cleaning out the nose and dusting with calomel constituted the local treatment.

Constant observation of these cases gave me an excellent opportunity of studying the specific power of calomel in diphtheria. In the oldest child the disease was undoubtedly kept in abeyance, and had we not intermitted the treatment the disease would never have produced any constitutional effect. In the beginning of the attack, I stopped the powders after she had taken them for twelve hours, and did not resume their use until the redness in the throat reappeared. After several days' use we again stopped them, thinking she was well, but found upon bacteriologic examination that her throat still contained bacilli. We put her back on the powders, and in two days her throat was entirely well and was declared sterile by the Bacteriological Department. I am convinced that had I continued the powders for four or five days from the time I first gave them, she would never have developed constitutional symptoms, other than the slight disturbance of circulation which she presented at the onset. With the recrudescence of the disease she became pale and lost her appetite, and showed some little depression of the heart-action. She never had, however, any membrane or any local indication of the disease other than redness and slight granulation of the pharynx. This, too, in spite of the fact that during the entire attack she was in close and constant association with the other cases.

The most phenomenal evidence of the power of calomel over diphtheria was, however, manifested in its action in the noses of the other three cases and on the membrane that had formed over the baby's ear. The very first insufflation of the noses of the two youngest children, with a thirty-three per cent. triturate of calomel, had so marked an effect in reducing the inflammation that I could not overlook or misinterpret the relation between the use of the drug and the result. After using the triturate a few times and finding no injurious results, I substituted the pure calomel, and after the first or second insufflation all discharge from the noses ceased.

The breathing, which had been very much obstructed in both children, became much better at once, and in the baby became normal and was never again obstructed. In the second child partial obstruction continued for a week or more. When all nasal symptoms had disappeared in the baby, we stopped the insufflation, but, as subsequent events proved, this was premature. After a few days, a slight discharge from the nose reappeared, and quite suddenly the temperature shot up to 104° and the heart became exceedingly depressed. We had the nose thoroughly cleaned out and insufflated with calomel, and in four hours the temperature again became normal and the heart's action improved. In cleaning out the nose, we found purulent discharge from both. The shock from this slight relapse kept the patient depressed for nearly a week.

In the second youngest child a recrudescence of the nasal symptoms also took place after several days with a rise of temperature and depression of heart's action. Although there was no nasal discharge, cleaning out revealed the presence of a small amount of purulent matter. After thorough insufflation the temperature became normal and all the symptoms again subsided. In both cases we now kept up the insufflations regularly and both rapidly advanced to recovery.

Another surprising effect of the calomel and a further evidence of its power over the disease was its action upon the enlarged glands. In the two youngest children the enlargement of the glands disappeared with the improvement of the nasal condition and this was as expected. In the third child we did not recognize the nasal condition at first and we therefore did not use insufflation. In her case the glands became exceedingly large and painful and although we gave the calomel internally and applied a solution of europhen in oil externally there was apparently no amelioration in their condition. Finally reasoning from analogy from the other cases we began insufflation and immediately the swollen glands decreased in size and in a few days they were normal.

As to the mode of action of the calomel when used in this way in the treatment of diphtheria I was at first inclined to think that it was by stimulation of cell-action in the throat, but these recent experiences leave no doubt in my mind that it is by local germicidal action. The frequent repetition of the dose keeps up a constant sterilization of the soil, and the smallness of the dose prevents evil constitutional effects of the calomel. In no other way can be explained the failure of the action of the calomel upon the membrane of the nose when given by the mouth alone and its speedy action upon the nose when used by insufflation. The phenomenal action of the calomel upon the membrane of the baby's ear points to the same conclusion.

It was a matter of no little surprise to me to find that calomel was not absorbed by the nasal mucous membrane. From four to five grains of calomel a day by insufflation produced no action upon the bowels so long as the occlusion of the nasal passages prevented the calomel from passing into the pharynx, but as soon as insufflation was attended with the passage of the calomel into the throat, as could be determined by the cough set up by inspection of the throat, it was followed by purging.

So far as my experience enables me to say anything about the size of the dose of the drug to be given by the mouth, I think the best rule to follow is to give as much as can be taken without setting up constitutional effects. I usually begin with a sixtieth of a grain and go up or down according to results. If the bowels become loose I decrease the dose and if they are constipated and I see no effect upon the local conditions in twelve hours I increase the dose. There seems to be a remarkable tolerance of mercury in diphtheria, and this seems to grow as the drug is given. I have reason to believe that a tolerance for the drug is also established by the disease, for I have found it necessary to give larger doses in recrudescences of the disease than in the beginning of the attack.

The drug can best be given rubbed up with sugar and should be placed

dry on the tongue. The secretions of the mouth will promptly distribute it over the pharynx. Whilst the child is asleep the powder can be placed inside of the lips without awakening it. I have found no trouble in having the medicine administered every fifteen minutes, night and day, and I have in no case found it necessary to have the child's sleep disturbed.

In conclusion I will but say that I trust others will try the treatment on the lines I have laid down. I feel confident that they will find in it a reliable method of dealing with this dread disease.

DISCUSSION.

Dr. S. Solis-Cohen stated that the use of preparations of mercury, especially the mild chlorid, in the treatment of cases of diphtheria, dates at least as far back as Bretonneau, whose favorite treatment for laryngeal diphtheria it was. Dr. Daly, of Pittsburg, has been one of those most enthusiastic in the recommendation of enormous doses of the mild chlorid in the treatment of pharyngeal diphtheria. Dr. Cohen's experience, while far from confirming the claim that calomel is a specific, an infallible remedy, has been highly favorable to the use of that drug in cases of pseudo-membranous sore throat of various kinds, given, however, in doses varying with the age of the child—somewhere in the neighborhood of one-eighth grain every hour, or more or less frequently, according to effect, until the characteristic stools show the child to be under the influence of the drug, then to substitute the corrosive chlorid in minute doses. Previous to the introduction of the antitoxin treatment, this had given him more satisfaction than any other routine method. He has had no experience with topical applications of calomel upon diphtheric membrane, although in common with others he has tried solutions of mercuric chlorid and has failed to find it so useful as the enthusiastic reports of certain French observers would lead one to suppose.

Dr. Cohen said that it is a little ungracious to doubt the diagnosis in the cases reported by Dr. Flick, especially as that diagnosis was made largely upon bacteriologic observations; but he had seen cases in which, clinically, he had no doubt of the correctness of the diagnosis of diphtheria, but in which the bacteriologic report was that they were not diphtheria; and he had nevertheless entered them upon his records as diphtheria. Again, he has seen cases in which, clinically, he had no hesitation in pronouncing against diphtheria; yet the bacteriologist has found Klebs-Lœffler bacilli present. As to the latter class of cases, although he has felt it necessary to comply with the health-laws and to take every precaution against spread of infection, in his own mind they still remain cases of diphtheroid throat and not of true diphtheria; and unless we are entirely to reorganize our clinical conception of the disease diphtheria, he did not see how we are going to get Dr. Flick's cases into it. The appearance of the diphtheric membrane upon the ear in one of the cases might be accounted for without supposing all the children to have had diphtheria. Dr. Cohen had seen many cases of inflammation of the nose and inflammation of the pharynx with inflammation of the glands of the neck, enlargement of the submaxillary glands, and purulent discharge, without being able to place them exactly in the nosologic category, and he has been satisfied to look upon them as infectious inflammations without designating them as cases of diphtheria. Some he has thought to belong to the group of less common manifestations of influenza; others have seemed to correspond with what German observers have described under the name of "glandular fever"; but that these cases are cases of diphtheria seemed to him very doubtful, especially in view of the fact that purulent discharges are reported in connection with them. In what has been said, there is no intention to challenge the correctness of bacteriologic observations or to dispute the etiologic relation of Klebs-Lœffler bacilli with diphtheria; but merely to intimate that bacteriologic diagnosis is not in itself sufficient, and that there still remain unsolved problems in the etiology of diphtheria.

Dr. Edwin Rosenthal said that before the advent of the antitoxin, he had always considered mercury a specific for diphtheria. As early as 1885, he had given calomel in diphtheria, but in rather larger doses than Dr. Flick. In laryngeal diphtheria he has given as high as five grains every hour—in fact he has given as high as 120 grains to a child five years of age, in twenty-four hours; nor did he notice that the child showed any evidence of the physiologic action of the calomel. He was led to believe that to ptyalize a child who suffered from diphtheria was an impossibility, and he not only gave such enormous doses of calomel to these children, but he has used at the same time corrosive sublimate in enormous

quantities, introduced 1-500 solutions into the child's nostrils, and swabbing out the fauces, in that way probably getting into the body of the child about a grain of corrosive sublimate inside of twenty-four hours.

In the case of a woman exceedingly ill with laryngeal diphtheria, the throat was swabbed out with 1-500 solution at intervals of from one to three hours, and in this way probably a gill of mercuric chlorid passed into the stomach; yet there was no evidence to show that the drug had any deleterious effect.

The insufflation of calomel brings up a very good point. In cases intubated and given calomel, the withdrawn tube was found encrusted with a substance which was probably either calomel or some modification of calomel. Dr. Rosenthal did not think it proper to wake a child every fifteen minutes to administer medicine, but that as much as possible should be given if the child shows any tendency to wakefulness. To give any remedy at too frequent intervals will endanger life and open the way to many complications. Dr. Rosenthal also spoke of the use of calomel by sublimation, rigging up a tent and burning calomel on a platinum pan and having child inhale the vapor. Two cases in which he had used sublimation died, but he has employed it in others with better success, the children recovering. From a fortieth to a sixtieth of a grain of calomel does not seem to be a very large dose, but when given every fifteen minutes, the child will be found to be taking a large amount without producing ptyalism. Whether or not the drug acts in the nose as a germicide through some chemic change, produced by the secretions, is something yet to be found out. At any rate, Dr. Flick's cases emphasize the fact that leaving the antitoxin out of the question, calomel is the best remedy in the treatment of diphtheria since the time of Bretonneau.

Dr. L. D. Judd said that he had his first experience with the calomel-treatment of diphtheria in 1882. This experience was based upon a letter of the late Dr. Reiter of Pittsburg to his friend, Dr. E. R. Squibb, and published in Squibb's Ephemeris, Vol. V, September, 1882. Dr. Reiter was known in his section of the country as the "calomel-doctor". His success in diphtheria was marvellous. Dr. Judd related that in the case of a child, eighteen months old, that seemed beyond all hope, he gave eighty grains of calomel within eighteen hours, and the child recovered. In the case of a lady, fifty-five years of age, weighing about 200 pounds, who was scratched upon the hand by a child suffering with diphtheria, the disease-process appeared at the site of the local lesion and also in the throat. Other measures having failed, the calomel-treatment was proposed as a *dernier ressort*. The patient was given twenty grains of calomel as the first dose and ten grains every hour for thirty-five hours. She took 365 grains before the characteristic discharge from the bowels took place. From the time she had taken about 100 grains she improved and continued improving right along with each powder. No throat-wash or inhalation of any kind was used; only a mild solution of potassium chlorate internally about every three hours. The woman made a perfect recovery. For a short time the muscles of deglutition were partially paralyzed, her gait was somewhat impeded, and the sense of touch was impaired, but under the use of electricity these symptoms rapidly disappeared. There was not the slightest evidence of ptyalism. Dr. Judd said further that he had never seen a case salivated through the calomel-treatment, properly administered, in diphtheria; neither hypercatharsis nor any other bad effect. Two essentials are necessary for success:—The nurse must be competent and reliable, and the characteristic dejecta must be secured, that is the "frog-spittle" stool, or that which has the appearance of "green polyps in an old water-trough."

Dr. R. G. Curtin said that in a number of malignant cases of diphtheria, in which large doses of calomel were given, beneficial results followed. In the case of a child eleven years old, who had taken about 100 grains of calomel in the course of twenty-four hours, the attending physician, being afraid that some constitutional effects might result, withheld the drug, and about twelve or fifteen hours after the symptoms recurred. The mother, having seen such beneficial results from powders, gave them again on her own responsibility, and marked relief followed within a few hours. A second time the calomel was stopped and again its renewal was followed by beneficial results. From experience with other forms of mercury Dr. Curtin believed that any form of mercury will prove beneficial in diphtheria. The advantage possessed by calomel, however, is that a larger quantity can be given with safety than of the bichlorid, or the biniodid. The only sequel that is to be feared after giving calomel is renal trouble. In cases presenting marked albuminuria, calomel does not prevent or modify the complication.

Dr. C. Pottberg related the case of a boy four or five years of age in whom the disease began in the pharynx and involved the mucous membrane of both nostrils. In applying hydrogen dioxid the parents had (owing to the struggles of the child) wounded the lips and gums to such an extent that the whole of the mouth was covered with membrane as well. After seven or eight days the larynx became involved, as indicated by hoarseness of the voice, etc., and the membrane had extended through the nostrils to the cheeks and around the mouth of the child. The child was now wrapped in a blanket so as to make him completely helpless, and laid over two chairs with

his face downward ; then with the hard rubber vaginal end of a syringe, at least one and a half pints of mercuric chlorid solution (1-5000) were pumped into the nares. As the child was lying face downward the liquid flowed out with the greatest freedom, thoroughly cleansing and clearing off the membrane. This procedure was repeated every three hours for a week and the case recovered.

Dr. William M. Welch expressed a great deal of confidence in mercury in the treatment of diphtheria. He has used calomel very extensively, but very seldom in the large and heroic doses mentioned by some of the speakers ; nor, on the other hand, has he used it in the very small doses recommended by Dr. Flick. Dr. Welch has frequently used calomel in doses of $\frac{1}{10}$ grain every half-hour in laryngeal cases, and has seen good results. Very rarely indeed has he seen any systemic depression or salivation from the drug, even when long continued. In this class of cases he has used calomel quite frequently by sublimation. This is done by placing the patient in an extemporized tent and burning therein fifteen grains of calomel every two hours for twenty-four hours, or longer if necessary. Good results appeared to follow the use of the drug in this way in laryngeal cases. Dr. Welch has much oftener used mercuric chlorid than any other form of mercury, and it has his confidence to a considerable extent, especially in the pharyngeal and nasal forms of diphtheria.

Only once or twice has he used calomel in heroic doses. In one instance as far back as 1891 he gave this drug in twenty-grain doses, not repeating very frequently, with a good result, as the patient who suffered from membranous croup coughed up a complete cast of the larynx and recovered. Two other children in the same family had just died from the same disease under more conservative treatment.

Not long ago there was admitted to the Municipal Hospital a white female child, aged ten and one-half years, who had been ill from diphtheria two days, and was croupy all of that time. It was stated that before the patient was sent to the hospital, the family physician had given her twenty grains of calomel every fifteen minutes until four doses had been taken. After admission intubation was performed, with marked relief to the respiration. After wearing the tube nearly forty-eight hours it was coughed out, together with a complete cast of the larynx in the form of an exudate, and the child was then able to breathe freely without the tube. Recovery resulted. Now, whether calomel had anything to do with the separation and expulsion of this cast is a difficult question to decide, as such casts are frequently coughed out when no calomel has been given.

Dr. H. A. Hare referred to the statement in all standard works on therapeutics for many years back, that children under seven years of age cannot be salivated by mercury. It should be remembered, therefore, that in giving large doses of mercury to children, neither salivation nor the other symptoms ordinarily looked for as a physiologic guide in giving this drug will be present. It is a well-recognized fact that, while children cannot be salivated by mercury in excessive doses, they can develop all the other disagreeable complications to which this drug gives rise—that is to say, the salivary glands seem to be the only parts which are not affected as is the rest of the body by this drug.

In a limited experience in the treatment of diphtheria in hospital practice, Dr. Hare has seen almost massive doses of mercuric chlorid apparently do a great amount of good. The subject, however, is one that requires research. Opinion is based at present almost entirely on empiricism. On one hand, while mercury tends to stop plastic exudation in diphtheria, as a general rule the mercurials are not to be given in diseases attended with asthenia. On the other hand, it may be possible that just as potassium chlorate, which is by no means as good a remedy, favorably influences some cases, mercury may, by altering the secretions of the larynx and pharynx and nasal cavities, make the mucous membrane an unfavorable site for the multiplication of the bacillus that causes the disease known as diphtheria.

Dr. Edwin Rosenthal added that there seems to be a unanimity of opinion that calomel in some way acts specifically upon diphtheria. But further there seems to be the same unanimity that the morbid process revives during the period that calomel is withdrawn ; that the length of the disease is not shortened ; that the temperature rises ; that the child again becomes ill ; and that the disease progresses despite the treatment. If, however, at this time, together with the use of calomel, the antitoxin be injected, improvement soon sets in and progresses to recovery.

Dr. Daniel Longaker stated that he had used mercurials with very satisfactory results. Since the advent of the antitoxin he has treated quite a large number of cases without a death, using corrosive sublimate largely. He raised the question, "What influence on the mortality has the excessive use of alcohol ?" Of course the results of the mercurial treatment are influenced by the action of other remedies that are used in conjunction with it. Dr. Longaker maintained that the excessive use of alcohol has a depressant influence, and that many cases are alcoholized or stimulated to death, and in consonance with this belief, he has largely abandoned the use of alcohol in these cases.

Dr. L. F. Flick stated that in administering calomel according to his plan there is no

necessity for awakening the patient, as a small powder can easily be placed within the lips of a sleeping child, and it will be swallowed. Dr. Flick has treated quite a number of cases by this method, in which the medicine was given every fifteen minutes for a week, or ten days even, and at no time has it been necessary to awaken the child or to disturb its sleep in the least. If the medicine is placed inside of the lips, the secretions take it up and distribute it where it ought to go.

As regards the diagnosis in some of these cases and its dependence upon bacteriologic examination alone, Dr. Flick said that the examination was made by more than one person. Clinically, the picture was not one of diphtheria, but the diagnosis was accepted, as it was based on repeated examinations. After the diagnosis had been made Dr. Flick learned that the older physicians knew better how to diagnosticate nasal diphtheria than we do. In an old edition of Meigs and Pepper on Diseases of Children, it is stated that, when a discharge from the nose and enlargement of the post-cervical glands is present, the case can be depended upon to be one of nasal diphtheria.

The discussion disclosed a striking fact, namely, that all who advocated the use of mercury, whether in the form of calomel or of corrosive sublimate, seem to have recorded the best results when they gave the medicine at short intervals. The result probably depends upon the short interval between giving the medicine. A rise or decline in the improvement of the patient can be noticed as the medicine is given or intermitted. Dr. Flick reiterated his conviction that the best results in the treatment of diphtheria with calomel can be obtained by constantly keeping up its germicidal influence. The rapidity with which the inflammation of the nose in the cases related and in one other case thus treated has disappeared appears to be proof of the specific action of the drug.

Dr. Flick agreed that alcohol is used too lavishly. He employs it himself in moderation, but is disposed to depend more upon strychnin.

HEMOTHORAX FROM STAB-WOUND OF AN INTERCOSTAL ARTERY OPERATION—RECOVERY.

JOHN S. MILLER, M.D.

[Read May 27, 1896.]

Mr. J. J., twenty-eight years old, was stabbed at one A.M., on December 2, 1895. The weapon used was either a penknife or stiletto, the wound being quite small. The hemorrhage was profuse, the blood saturating the man's clothing. He walked unaided to his home, three blocks away. After being placed in bed the bleeding ceased. At two A.M. he was removed to the surgical ward of St. Joseph's Hospital, when it appeared that the wound was not of a serious nature. At nine A.M. I saw him for the first time and found him in great distress. The temperature was subnormal, the pallor was extreme, the pulse imperceptible at the wrist, and all the symptoms suggested traumatic anemia and internal hemorrhage. At this time it was difficult to decide whether anything could be gained by attempting to find the bleeding vessel. The man was barely able to make his ante-mortem statement to the police authorities. With the use of strychnin, whisky, heat, etc., however, he was raised to a more hopeful condition.

On examination a small punctured wound was found at a point two inches to the left of the middle line of the vertebral column and on a level with the inferior angle of the left scapula. There was no bubbling of blood and air, which fact may have been misleading to those who saw the case on admission. On auscultation there was an absence of respiratory sounds over the entire left thorax. There was percussion-dulness over the entire area of the lung.

A diagnosis of internal hemorrhage from an intercostal artery was made and the skin-wound was enlarged to about two and one-half inches. It was found that the intercostal muscles had been penetrated by the weapon. When the finger was forced into the thoracic cavity there was a gush of at least two quarts of dark blood, which was allowed to drain out by placing the patient partly on his back at the edge of the table. The divided ends of the intercostal artery could not be found, especially as there was no bleeding at this time. The inferior border of the rib had a crescentic defect, as though the weapon had been turned after its introduction. Exploration with the eye and finger showed that the lung had collapsed to about one-third its normal size. Evidently the lung had not been punctured. The wound was firmly packed with iodoform-gauze, which was removed in forty-eight hours, when there was another gush of very dark blood. This blood had not accumulated since the packing had been placed in the wound, as the gauze was not stained at the point from where the hemorrhage would have come; nor did the patient exhibit any signs of renewed hemorrhage. The blood had evidently remained owing to imperfect drainage.

The rest of the history was uneventful, until two weeks ago, when a large abscess was discovered over the site of the stab-wound. To-day I explored the sinus and found that the rib is either carious or necrotic at that point. Although the immediate indications called

for intra-venous or subcutaneous infusion of normal salt-solution, yet I feel that in this case it would have proved detrimental to have resorted to infusion with a view to strengthening the patient for an operation in order to ligate the open vessel. I determined to first secure the bleeding point, and then to practise infusion should the indication arise, as it is at this time that any benefit may be derived. As I understand it the saline infusion only acts mechanically and gives the heart its usual work to do; it also dilutes the remaining blood and thereby lessens its coagulability. If this is true, it would appear unwise to increase the blood-pressure and thereby force the remaining blood out through the divided vessel or vessels. Therefore, I maintain, as has been illustrated in this case, that our first effort should be directed to stopping the leak and then introducing the physiologic circulating medium.

THE EXAMINATION OF CULTURES FROM CASES OF SUSPECTED DIPHTHERIA.

B. MEADE BOLTON, M.D.

[Read June 10, 1896.]

Owing to coöperation on the part of many of our physicians, it has been possible to accumulate certain data in regard to the bacteriologic examination in cases of suspected diphtheria. A full report will be published by the Board of Health, but it may not, perhaps, be amiss to bring before you in advance some of the more interesting and instructive points that have been developed.

From May 30, 1895, to January 1, 1896, there were examined 1421 primary cultures and 1942 secondary, making a total of 3363 cultures. Of the 1421 primary cultures 1207 were made from the throats of persons showing clinical evidence of diphtheria and 214 were made from the throats of healthy persons who had been exposed to infection. Of these cases only those in which the physician made a diagnosis are of value in comparing the clinical with the bacteriologic diagnosis and this comparison shows a remarkable agreement, all things considered. The diagnosis of diphtheria was made by the attending physician in 557 cases; in the remainder of the cases the physician either stated that the case was not diphtheria or left the matter in doubt. But in the 557 cases diagnosticated as diphtheria bacteriologic examination showed the presence of diphtheria-bacilli in 507, or 90.2 per cent. In other words at least ninety per cent. of cases pronounced diphtheria by the attending physician had the Klebs-Lœffler bacillus present.

In 148 cases the physician stated that the disease was not diphtheria. The Klebs-Lœffler bacillus was found in forty of these cases: the clinical and bacteriologic diagnosis agreeing consequently in 72.9 per cent. According to this it would seem that in cases of angina which do not show sufficient evidence clinically to be called diphtheria 27.1 per cent. have the same organism present that is usually found in clinically typical diphtheria. Those who call all anginæ caused by Lœffler's bacillus diphtheria would regard these as mild or atypical cases of the disease.

If the 557 cases in which the physicians pronounced the disease diphtheria be taken with the 148 that could not be called diphtheria clinically it will be found that the clinical and the bacteriologic diagnosis agree in 86.4 per cent.

Cultures were taken in 214 cases from the throats of persons who had been exposed to diphtheria, but who presented no clinical symptoms. Of these 214 cases, 89, or 41.5 per cent., showed the presence of the Klebs-Lœffler bacillus; 95, or 44.3 per cent., did not show the bacillus and the others were unsatisfactory. It seems, accordingly, that more than one-third of persons more or less exposed get the bacilli in their throats. It would be interesting to know how many of these persons subsequently develop the clinical symptoms of the disease. In fifty of these cases it was possible to determine that the ba-

Diagnosis of diphtheria given.	Diagnosis of diphtheria confirmed by culture-test.	Diagnosis not diphtheria.	Diagnosis not diphtheria confirmed by culture-test.	Total number of cases in which diagnosis was given.	Total number of cases in which the diagnosis was confirmed by culture-test.
557	507 or 90.2 per cent.	148	108 or 72.9 per cent.	705	615 or 86.4 per cent.

Cases of persons exposed but presenting no clinical symptoms.	Bacilli found.	Bacilli not found.	Unsatisfactory cultures.		
214	89 or 41.5 per cent.	95 or 44.3 per cent.	30 or 14 per cent.		

cillus persisted for 13.3 days on the average.

In 460 cases presenting clinical symptoms of diphtheria the length of time that the bacilli were present, dating from the appearance of the first symptom to the disappearance of the bacilli, could be determined. It was found that this varied from seven to ninety-six days, the average being 28.3 days, irrespective of treatment. A tabulated summary of the results detailed is hereto appended.

DISCUSSION.

Dr. A. A. Eshner said that it must be very gratifying to the members of the profession in this city that there should be so striking an accord between the clinical and the bacteriologic diagnosis. All clinicians must recognize that the diagnosis of diphtheria today is attended with certain difficulties from the fact that there are many affections of the throat—of the tonsils, of the pharynx and of the adjacent structures—that clinically present manifestations closely simulating, if not actually identical, with those of diphtheria locally, but lacking the virulence of this disease and lacking the profound constitutional intoxication of diphtheria. It is likely that many of these affections, like diphtheria, are infectious, are transmissible disorders, but differing from diphtheria etiologically. They may be designated diphtheroid if one chooses, *i. e.*, resembling diphtheria, but depending on other causes than the Klebs-Lœffler bacillus.

It must be borne in mind in connection with the nature of a doubtful case that diphtheria may have existed and at the time of examination diphtheria-bacilli fail to appear. That this may take place has been shown in some instances in which, while diphtheria-bacilli were not found at the side of the original lesion they were demonstrable in the secondary lesions. At best, failure to find the organisms cannot be accepted as conclusive of their absence. On the other hand the bacilli may be present and yet the symptoms be so mild as to obscure the recognition of the disease. Another point of great importance as regards these examinations is the long period during which the micro-organisms remain lodged within the air-passages in the situation in which the disease appears, or otherwise, and throughout which period of course these cases are capable of transmitting infection. There is an abundance of clinical observation to show that diphtheria-bacilli may lodge at the site of the disease for many weeks and the organisms that are obtained from these situations are still capable of generating virulent diphtheria. Our notions of the pathology of diphtheria have been very much confused by the introduction of the term pseudo-diphtheria-bacillus, together with what the use of such a term implies. It will be admitted by all impartial observers that diphtheria is due to the diphtheria-bacillus, the Klebs-Lœffler bacillus, and to no other micro-organism, although it is recognized that lesions very closely simulating the diphtheric lesion may result from other influences. It is very likely that many of those which in the past have been designated as pseudo-bacilli were actually diphtheria-bacilli although for some reason or other of lower pathogenic activity.

Dr. Bolton said that it had been very properly stated that in some cases there are anginæ which very closely resemble diphtheria but which are caused by a different micro-organism. Recently an article appeared in the

*Annales de l' Institut Pasteur** which goes to show that only two organisms are ever concerned in these anginæ—either the Klebs-Lœffler bacillus or the streptococcus. A good deal of work has been done in the past that went to show that all the pus-organisms and a great variety of other organisms might cause these anginæ, but in this special study, which was carefully made (the seat of the lesion being first sterilized with hot glass so that no surface-complication entered), in all of these cases the streptococcus was found, mostly in pure culture, in five cases associated with the colon-bacillus and in eleven cases associated with the staphylococcus pyogenes, but the author accounts for this by unintentional contamination. This work goes to show, and it is so interpreted, that only the streptococcus is concerned when the Klebs-Lœffler bacillus is not. If this dictum be accepted, according to the statistics presented ninety per cent. of cases that have clinical symptoms of diphtheria are caused by the diphtheria-bacillus and ten per cent by the streptococcus primarily. After the streptococcus has engendered its evil effects other contaminating organisms enter into the morbid process.

It is scarcely worth while to dwell upon the question of pseudo-diphtheria-bacilli. It is pretty well proved that they are really the attenuated diphtheria-bacilli.

*Lemoin: Contribution a l'etude bacteriologique des Angines non-diphtherique. *Annales de l'Institut Pasteur*, No. 12, Vol. 9, 1895. Examination of 165 cases showed the streptococcus present in every case, for the most part in pure culture.

THE PROFESSION, THE OPTICIANS AND THE PUBLIC.

EDWARD JACKSON, A.M., M.D.

[Read June 10, 1896.]

The use of correcting glasses is still extending, even where it is now most common. Their more accurate adjustment to the needs of the wearer is likely to still farther popularize them.

The employment of the eyes for the seeing of minute objects at short distances becomes more general and constant. A multitude of new stimuli tend toward the greater refinement and delicacy of the average nervous system.

It can be safely asserted that the need for glasses will continue and extend until there occurs some important change in the direction of our social development.

Who shall measure the eye for glasses? is then a question of some importance, and one about which the medical profession should be able to arrive at some common opinion. It is a question that may be considered from different points of view, and it seems appropriate for discussion in a meeting representing all parts of our profession.

I have no doubt that in the end this work will be done by those who can do it best and cheapest. Only long experience will demonstrate to some just who these will be; but from my knowledge of the requirements of the work, I believe that it will ultimately be done by a class of practitioners familiar with the general principles of medicine and surgery, experts as to the diseases of the eye, and specially trained in the methods of measuring refraction and conditions of ocular muscular balance.

A little over one generation ago, prior to the publication of Donders' great work on the *Refraction and Accommodation of the Eye*, the science and art of adjusting glasses for the correction of ocular defects did not exist, and the rudiments of that science and art were held chiefly outside of the medical profession. Practically, each person who got glasses fitted himself with them, with the aid of such hints as the vendor, from his experience, could give. Under these conditions, the use of glasses was comparatively limited.

The new knowledge and skill found the medical profession best prepared to receive them, and in best relation to the community to utilize and apply them; but the subject, naturally, also attracted the attention of those who had heretofore given the public the little help it had in the selection of glasses; and the opticians did not fail to see the advantage of their position and the plausibility of their claim to this sort of work. Had they possessed as broad a training, and had they been as much actuated by the spirit of scientific investigation as the medical profession, they would, at least for a time, have kept the confidence of the public and gotten a larger share of the new work. As it is, we are still in a transition-state. Many fail to recognize which way things are moving, and some are earnestly and conscientiously trying to turn back the current of events; while others seem not to recognize that they are moving at all.

The ophthalmologists have not failed to develop and put forward their views upon the subject; yet I believe the reasons supporting those views are even

stronger than they have yet been made to appear.

In reply to the statement that every pair of glasses has a medical bearing, it may be urged that every meal one eats has a medical bearing, every piece of underclothing one wears has a medical bearing: yet one does not run to his doctor each day with his bill of fare, or go to him for a prescription when he wants a new undershirt. Very true; but the varieties of undershirt one has to choose between are very few; and each individual, when he comes to choose for himself, brings to the choice some considerable experience of the effects of undershirts. Again three or more times a day, from birth, each person has been experimenting in dietetics. Generally some of his most vivid and earliest remembered impressions have been of the results of his investigations in this field of knowledge.

Probably it is to this practical experience of the effects of food upon our own particular bodies, more than to anything else, that reference is made in the saying that "at fifty every man is a doctor, or he is a fool."

The indications for food and clothing are commonly strictly physiologic. Let indications regarding them arise, however remotely, in the domain of pathology, and how quickly not only the food we eat and the clothing we wear, but even the water and other fluids that people drink, and the air they breathe become the subjects of the physician's prescriptions, without question or wonder.

Now, the possible variations of glasses to be chosen from are practically infinite, and the indications for their use almost aways lie in the domain of pathology.

Presbyopia may be physiologic; its earliest indications may be physiologic, although always new in the experience of the individual; but these first indications are commonly disregarded; and not until pain or inflammation has occurred, and become sufficiently serious, does the presbyope come to think of glasses. In hyperopia, in astigmatism, in myopia, pain or some other disorder of the nervous system, or some pronounced disease of the eye, or both, are the indications to meet which glasses are required.

The general association of ocular disease with the need for glasses is felt by every thoughtful and conscientious refracting optician.

Scarcely a month passes that I do not receive an inquiry from some optician as to how he can learn something of the diseases of the eye to enable him to better do his work as a refracting optician. Generally, the only course I can indicate to him discourages him; he has gone too far on the wrong track. But within a month I have talked with a man of fifty, a practical optician all his life, who has felt so much the need of this knowledge that he has entered upon the now-a-days thorny path that leads through a four years' course to the medical degree, simply to acquire this desired acquaintance with ocular disease, which he purposes to use in the fitting of glasses.

Again, the more enterprising optical firms that make a point of "examining eyes free," equally emphasize the point that these examinations will be made by "regularly graduated physicians." So far as I know, every opticians' school in the country—and there are a host of them—is taught by a "regularly graduated physician."

Since the opticians themselves have so completely given away the case as to the importance of medical knowledge to the adjuster of glasses, it might seem superfluous to argue it at length before a medical society. But, really, the opticians are more keenly alive to the medical aspects of a pair of glasses than some of the medical profession. Rip Van Winkle, M.D., cannot see why a man should not go into a store and buy a pair of glasses now, just as his grandfather did two generations before him. Indeed, a medical journal that claims the largest circulation of any weekly medical journal in the country, and which considers itself very bright and up-to-date, speaking editorially of people who suffer from hyperopia, myopia, and astigmatism, but who "cannot afford to go to a specialist," says: "Now, we advise such to go to their

family doctor and let him decide whether the optician is sufficient." (*Medical Record*, November 24, 1894.) There is in this city a refracting optician who claims to refract people referred to him by two hundred of its doctors. The claim is without doubt a gross exaggeration, but I have heard from patients—who had learned by sad experience that the advice was not sound—that they had been advised by their family physician to go to this optician. So, perhaps, this subject is not out of place in the Philadelphia County Medical Society.

Another reason why the work of measuring for glasses should be done by a special class of professional men is that it requires time, thoroughness, exactness. The benefit derived from a pair of glasses is not in proportion to their approach to accuracy of correction. There is often no benefit whatever until the approach to perfect accuracy is very close. Nowhere is painstaking attention to minute detail more productive of benefit. The public may be slow to learn, but ultimately it will learn that the best service in this matter is the only kind of tolerable service; and this kind of attention it will have to pay for. If it is rendered by the refracting optician, as in rare instances I believe it now is, he will charge for his time or he will retire from the business. A refracting optician who came to consult me about wearing glasses himself told me that his plan was to charge twenty-five dollars for a pair of glasses and the fitting of them. The glasses, in gold frames, cost him from two to four dollars. The difference represents a very good fee for skilled services. Some doctors follow this same plan of charging a lump sum for glasses and services, believing that patients will more readily pay this than a professional fee; but the net result to the patient's pocket-book is the same; and in the long run I believe the more frank and truthful plan will grow in favor.

The proper relation of the optician to the prescriber of glasses and to the public is then closely similar to that of the apothecary. He has at present, however, some advantages over the apothecary. While the latter finds his field for the exercise of especial skill more and more encroached upon by the wholesale manufacturer of standard drugs and proprietary preparations, the optician has not only had the opportunity extended for the exercise of his technical skill in the grinding and mounting of lenses, but there has also been opened to him a new field in the fitting of frames to the peculiarities of the individual face—a work properly remunerative and as yet very rarely understood and imperfectly appreciated by the opticians themselves.

This relation of the doctor and the optician to the public is liable to the same abuses as that of the doctor and apothecary. Collusion by which the optician gets patronage and the doctor gets a percentage on the optician's receipts is the same mean, contemptible sort of theft as similar collusion between doctor and druggist; and it is, perhaps, equally common and more unblushing.

An optician in another city recently informed me that doctors had three plans with reference to the glasses they prescribed. A few merely concerned themselves with seeing that the glasses furnished by the optician were the glasses ordered, and that the frames were well fitted. A larger number bought the glasses of the optician at wholesale rates, and sold them to the patient presumably at retail rates, or for a lump sum including professional services.

I have heard of the indignation of the students of a certain medical college when they learned that their professor, who followed this plan and was supposed to supply the glasses at cost, had been pocketing an extravagant profit.

The third plan was for the doctor to send the patient to the optician, who fitted the frames and charged a full retail price for the glasses, and then credited the prescriber with a large fixed percentage thereof, or with the difference between the wholesale and retail prices, and at stated intervals sent the doctor a check for the amount.

I have reason to think that the same thing occurs in Philadelphia. I have had hints that such an arrangement was possible in various quarters, and have had the whole plan of fraud explained to me by one especially eager to perpe-

trate it on my patients if I would make them his customers. I have seen what appeared to be photographs of checks and stubs from the check-book of a firm of opticians, representing the payment of such commissions to a member of this Society. In this case I offered, if those who claimed to know the facts would testify before the Censors of the Society, to take upon myself the unpleasant office of preferring charges; but no one was willing to appear as a witness. One of the opticians, hearing of my offer, came to me to threaten prosecution for libel. It occurred to me that this would be just the thing, that in a court of justice one could compel unwilling witnesses, and I tried to encourage him to make good his threat; but he never showed any inclination to pursue the matter further.

This incident helped to convince me of the utter inadequacy of our Code of Ethics, and the machinery for its enforcement, to secure even common decency on the part of those members of the profession whose natural tendencies are in the opposite direction. I think, though, that it does good to discuss such matters in a meeting like this. It certainly must make one uncomfortable to hear his detestable course of action properly characterized, and not dare to say a word in defence of it, lest he should brand himself as one of those lacking in common honesty.

The open discussion of such matters leaves no excuse for any to sin through ignorance; and as the relations of the oculists and the opticians to the public become more settled and better understood, we may hope that such abuses will diminish. Perhaps it will help matters to regard with suspicion the prescriber who has all his prescriptions filled at a certain store or stores; and not to recommend the optician who is known to seek to extend his business by such illegitimate methods.

If it be desirable that the measurement of the eye for glasses should be done by a special class of practitioners, how shall it be brought about? In this day the first thought of many will be, by special legislation directed to that end. This, I think, is quite impracticable. Other reasons against it might be urged, but this alone is sufficient. There is not any sufficiently defined class of persons in the community to whom the right to prescribe glasses could be confined by legal enactment. It has been suggested to confine it to legal practitioners of medicine; but the mass of medical practitioners are not competent to do this work, and to confer upon them the especial right to do it on any other ground than that of peculiar fitness would be a gross violation of legislative principles. Nor is special legislation needed. I believe that by giving better and cheaper service in this direction of fitting glasses, and putting itself in communication with those who need such service, that portion of the medical profession doing this work will get practically all of it to do.

The great reason why people go to opticians to have their eyes fitted is that some doctors do not do any better work in this direction than the opticians. Apparently some of those engaged in ophthalmic practice, and some who are on other accounts justly regarded as leaders in the profession, think it beneath their dignity to apply their attention earnestly and seriously to the measurement of refraction. They cannot stop to give a "refraction-case" time and careful attention, to apply to it diversified and repeated tests. They prefer rather a few hasty and superficial trials, a jump at a conclusion, an assumed air of wisdom, and a dogmatic opinion, often ridiculing the idea of benefit from accuracy in the adjustment of glasses.

From the offices of such prominent practitioners, and their more numerous imitators, goes out a steady stream of patients who know by their own experience that doctors cannot fit glasses any better than the opticians; and the impression they make on public opinion can only be eradicated after the removal of its cause.

I am glad to say that there is less of this influence to be combated in Philadelphia than in any other large city that I know of; still we feel it here. It will have to be recognized that good work is never done by one who does not think the work is worthy of his best effort,

and that the measurement of the refraction of the eye is something apart from other branches of medical diagnosis, requiring its own special apparatus, special methods, special training and experience.

A few words as to the cost of this service. Evidently the editor of the *Medical Record* makes the supposition that there exists a large class of the community who have a "family physician" but "who cannot afford to go to a specialist" the occasion for his remarkable utterances on the usefulness of refracting opticians. The patient who could not afford to go to a specialist until he had spent considerable money for glasses at the optician's, or lost days from remunerative employment in waiting on the service of a public clinic, and who then comes, anxious to have the thing "done right," is a very familiar figure in the office of the ophthalmologist.

The fees of the expert of large reputation and full practice are larger than those of the average practitioner in any branch of work, and if by specialist this kind of practitioner is always meant, it is one matter; but it is not more necessary that all cases of refraction should be measured by the famous teacher or great hospital surgeon than it is that such should set all fractures or treat all cases of heart-disease. There exists in ophthalmic practice a mass of younger, less known, but well-trained and conscientious workers, who are thoroughly competent to do such work, and of whose existence and merits the general practitioner should inform himself, as he would be the first to deny to these professional brethren the advantage of those methods of advertising by which he becomes acquainted with the claims of opticians.

Let this also be borne in mind: The specialist in ophthalmology knows none of the circumstances of the home or the business-relations of his office patients, such as the family physician can observe in his visits to them. Hence his only proper course of procedure in justice to himself is to charge a full fee, even when, were he better informed, he would make a reduced charge. If the family physician will bear this in mind and inform the oculist of the circumstances of his patient, much of the apparent reason for thinking the latter illiberal, or extravagant in his charges, will vanish.

DISCUSSION.

Dr. William Thomson said that his experience justified what Dr. Jackson had said in regard to the need for accuracy and care and also that no man can do excellent refraction-work unless he has the temperament for this work and a certain kind of fitness. Dr. Thomson was one of the first in this country to turn attention to the careful study of refraction. When he first commenced to practice there were clinical facilities, but little attention was paid to the correction of optical defects.

In regard to getting rid of all errors of refraction fairly and within a reasonable time and then to attempt to solve the problem of binocular vision, not of giving the patient merely sight, but of giving him power to work from eight to ten hours a day, is a problem that is worthy of study and one that the ophthalmologist attempts to solve every day in his practice. The ophthalmologist has not commenced to do his best work, the best he is capable of, because the public is unwilling to give him an opportunity to do it and to recompense him accordingly. It is almost impossible after going through a long and tedious examination to get rid of every particle of defect that can be demonstrated or corrected.

In selecting an optician one is to be preferred who sells glasses only upon the prescription of a physician. Such an one is likely to do better work and can be depended upon for accuracy and care.

In New York a project has been agitated for the organization of a national body of some kind that will have the right, perhaps, to make examinations and give degrees so as to place the control of the measuring of refraction in the hands of persons who are not medical people. Of course the first thing to be done would be to give these men the right to use a mydriatic.

Perhaps the more that question is agitated the sooner it will come before the body of the profession in such a way as to enable those

who are trying to do scientific and satisfactory work to get their sympathy and their support. The ophthalmologist is not in any sense a specialist alone. He tries to keep himself as well fitted to be a doctor as if he were a general practitioner. There are very few men who have won their spurs as specialists who have not done general work and who could not do this again if necessary. So in this question as to whether refraction-work should be done by men set aside by temperament and opportunity or whether it should be done by so-called opticians, the medical profession will stand with the best men within its ranks who are to-day known as ophthalmologists.

Dr. L. J. Lautenbach said that Dr. Jackson had approached his subject in a sincere, thorough and proper manner. The matters of which he complained come to a natural solution sooner or later, but this arrives the sooner the more the questions are discussed. The paper distinctly points out how this result is to be reached.

As a matter of history, it is known that some of the profession, in times past, received dividends from druggists. This practice has pretty well disappeared, and it is doubtful whether the number of such cases in Philadelphia to-day amounts in all to more than a handful. Within the last thirty years, this commercial spirit has disappeared more and more, as the sense of honor must ever grow with knowledge.

The matter of opticians being in collusion with eye-physicians is of much rarer occurrence now than it was ten or twenty years ago. That it exists is to be believed, and that opticians offer a certain percentage of their profits to the recommending oculist is too often charged. Dr. Jackson omitted another business matter to which opticians sometimes resort, viz.: Overcharging. There are some opticians in the city of Philadelphia who, when they know that a man appreciates what a thing is worth, will charge him a fair price, and when they have one who does not know what is a fair price (and the poor are the least likely to know), they make much larger profits by charging an extortionate and unjust price. This is done not only by some known to be unreliable, but even by some who are supposed to be extremely reputable. The consequence is that the very people who should deserve from the optician the greatest service for the least money pay the greatest amount of money for the least service. It is well known that the wealthy are the most exacting and demand from the optician the maximum of time and trouble. The class of patients to-day who are most eagerly sought for by opticians are those from the hospitals, because they pay cash and give the least trouble, and, knowing less of the value of frames and glasses, are the more easily imposed upon.

In Dr. Lautenbach's opinion a man is justified, when he writes a prescription for glasses, in sending the patient to someone who can be depended upon—not only that the patient be not overcharged, but that he may have the glasses accurately ground and the frames perfectly fitted, with the assurance that the optician will be willing to make any corrections necessary without extra charge, and will keep the frames perfect in fit in the future. It is proper and right, and an advantage to the patient, that every eye-specialist should designate a series of opticians. He should not allow the patient to go out among them unadvised. He should give the patient the advantage of his knowledge in this respect as well.

The cost of professional services may be materially increased simply through the mistakes of the optician. Thus it may be necessary to return the glasses many times to the optician before the glasses and their fit are correct.

It is not only the right and privilege of the ophthalmologist, but it is decidedly an advantage to the patient that some one or more opticians should be designated, upon whose ability and accuracy reliance can be placed. The public is becoming educated to the necessity of having its refraction-work done by a physician skilled in the specialty, in preference to one, not a doctor, who is only versed in the mechanics of the eye and the adaptation of glasses. Physicians, too, are becoming more and more aware that eye-specialists are not robbers; that they are willing to make concessions to the poor and those in moderate circumstances. It is not only that eye-physicians have striven with all their might to advance their specialty and broaden their knowledge, but of late there has been a more liberal spirit amongst the general profession. Twenty or twenty-five years ago the specialist was considered almost, if not entirely, outside the pale of pure medicine. It was thought that specialism was going to take work out of the general practitioner's hands. The effect has been quite otherwise, and the general practitioner realizes more and more that there is a place for the specialist, their functions being separate and distinct; that they both do work and can work not only for the good of the patient, but for the profession, as well as for themselves.

Dr. T. B. Schneideman emphasized the fact that the measure of refraction is more than a measure of refraction. It is a measure of the eye, and the physician who makes refraction gives information as to the condition of the nerve, choroid, media, and so on. In that way it might be advisable to go to the man with a better reputation than to go to the man able to do refraction alone. Every refraction done by the oculist should be an index as to the general condition present.

THE SURGICAL TREATMENT OF INSANITY.

ERNEST LAPLACE, M.D., LL.D.

[Read June 10, 1896.]

While it is evident that a surgical procedure must not be resorted to in every case of insanity, still it is also evident that certain cases of insanity are due to impaired mental function resulting from a removable cause. In its widest sense, insanity is as vague a term as the word fever. Just as fever means a symptom of many disturbances from various causes, affecting certain trophic centers of the brain, removable or modified according to the nature of the fever, so also does insanity imply to me the evidence of improper function of the intellectual centers of the brain. I am prepared to consider it as a symptom of a *cause* which on careful study of the case may be determined as removable, or not.

The art of cerebral localization has advanced to the point of enabling us today to locate with a fair degree of precision the cause of impaired function when existing in the motor or sensory area—not so, however, should the intellectual area be affected. In this regard we are still backward; hence the total inability to locate precisely the part affected in the vast majority of cases of insanity and the confinement of the mentally diseased in asylums, where, unfortunately, too little is done to rescue them from their living death.

There are cases, however, in which the history of traumatism distinctly exists as the predisposing cause of the insanity. Let there lurk in the system a tuberculous, syphilitic, gouty or other diathesis, and the traumatism producing upon the area of the brain affected a spot of least resistance, an inflammation results, acute or chronic, which will temporarily or permanently alter the function of the area affected.

The brain, encased in its bony box cannot be seen and treated when affected by disease or traumatism as any superficial portion of the body can. Nevertheless, it is just as likely as the rest of the body to be the seat of pathologic phenomena, which elsewhere, would be met and checked by local treatment. It is to enforce this idea by the practical results already obtained, that I desire to relate the following cases.

Case I. J. F., aged thirty-four, residing in Philadelphia, was struck on the head two and one-half years ago by a falling brick. Subsequently he suffered from severe headaches; although at no time was there any outward sign of injury. Within two months he became insane and was then confined in the Philadelphia Hospital for the Insane during six months. He was taken back to his home, when I was called to see him for a totally different trouble. The history of injury to the skull, together with a few spots on the body, suggested to me the possibility of syphilis complicating whatever harm to the brain a traumatism of the skull might have produced. I, therefore, trephined over the seat of injury, on July 20th, 1895, at St. Joseph's Hospital, nine months after the insanity had declared itself. A button of bone one-half inch in diameter was removed over the parietal eminence, and the dura was found white and thick, and quite adherent to the button. The rongeur forceps was used to remove bone above and below to the extent of an inch and one-third inch wide. The

dural separator was inserted so as to separate the dura from the skull anteroposteriorly, above and below, over the whole area of the left hemisphere. The patient withstood the operation well, and had neither pain nor fever following the operation. He was immediately placed on specific treatment. During the three weeks after the operation he improved gradually, recognized his relatives, and six weeks after the operation he left the hospital having entirely recovered his reasoning faculties.

Here the traumatism, without producing a fracture of the skull, had caused a local development of syphilitic phenomena, which, after surgical interference, became absorbed under specific treatment, thus restoring the impaired intellectual centers to their proper function.

Case II. Mrs. D., aged fifty-four years, married, was struck by her husband with his fist on the left side of her head. A short while afterwards she showed signs of dementia, and gradually became violently insane. She was under restraint during one year in Shenandoah, Penna., where she lived. She was being taken to the Pennsylvania Hospital for the Insane when I was asked to see her. She was in a great state of excitement or violent delirium, causing so much disturbance as to be handcuffed and isolated. She was trephined in the left temporal region, at a spot where her son told me she often placed her hand, although she never complained of pain. The bone was found much thickened; the diploe was fully one-fourth of an inch in thickness and from it came about four ounces of sero-sanguinolent fluid. There seemed to have been a hyperplasia of bone in this locality. We finally reached the inner table and concluded that the skull in that region was at least twice the normal thickness. The dura was very much congested; it was carefully separated, and the parts were packed with iodoform-gauze. On the following day the patient had completely quieted down, and would remain in bed without the straps used to restrain her before the operation. Within a week her reason was entirely restored and three weeks after the operation she returned to her home absolutely well, and is so still, at this moment, six months after the operation.

Case III. J. W., a young man, aged twenty-six years, suffered for four months from melancholia amounting to insanity. His sister had committed suicide four months before, after he had quarrelled with her, and thenceforth he was pursued by the thought of having caused his sister's death. He sank into the deepest melancholia, meditating suicide and unwilling to work or even talk to anyone. His melancholia had, however, started over two years ago and became aggravated by his sister's misfortune. There was furthermore a dulness of intellect amounting to what might result from a chronic meningitis. The patient was trephined in the left temporal region and a strip of bone, one-quarter inch wide, was removed from the anterior portion of the skull, corresponding to the coronal suture. Considerable trouble was experienced in checking the severe hemorrhage that occurred during the operation, the patient being no doubt somewhat hemophilic. On regaining consciousness after the operation he was told how absurd it was to think that he was the cause of his sister's death, and in the course of a few days he began to take an interest in matters in the ward of the hospital, gradually becoming brighter and helping the nurses. One month after the operation, on November 5, 1895, he left the hospital, feeling perfectly well, making no further allusion to his sister's death, and desirous of returning to work.

In this case there was no history of traumatism, but simply a functional disturbance very much on the general lines of so-called *idiopathic* diseases, which remain so classified only so long as science does not place them where a true knowledge of their cause would entitle them to be.

Case IV. J. O'D., aged forty-five, a laborer, had been well all his life. He was seized some ten months ago with symptoms of "delirium of persecution."

He imagined that people on the street were conniving for his destruction. He saw in all advertisements of newspapers

the schemes of people who wished to estrange his wife from him. He complained of constant frontal headache. He refused to work. Being taken to me by his brother, I advised a craniotomy over the region of the headache. This was acceded to. On May 22, 1896, I performed the operation and found the dura quite adherent to the skull. The adhesions were carefully separated. The operation was very bloody. The patient has not shown signs of any delusions since. He is about to return home, not knowing precisely why he came to the hospital, and denying that he ever thought that he was haunted or persecuted by anyone.'

I venture to offer the following conclusions:

1. Traumatic insanity is dependent upon an appreciable pathologic condition incident to the traumatism and interfering materially with intellectual function.

2. Idiopathic insanity is dependent upon an inappreciable alteration of the brain-substance interfering with intellectual function.

3. The relief of pressure by trephining and extensive craniectomy is a harmless procedure. It is a most valuable adjuvant to promote, with the aid of suitable medication, the absorption of deep exudates and the drainage of fluids from the cranial cavity, which would otherwise be retained and act as irritants.

4. The foregoing cases speak for the accurate diagnosis and prompt surgical interference in such cases of insanity as follow traumatisms. When we remember that the brain in its thick meninges is furthermore encased in the skull, and how it is exposed to the same pathologic reactions as the rest of the body, we will feel less loath to interfere so as to apply to the brain and the meninges the same common-sense rules of surgical treatment as are applied to the rest of the body; but to do this, we must reach the parts affected.

DISCUSSION.

Dr. A. J. Downes related the case of a young woman who had suffered intensely from right-sided headache and was melancholiac and depressed, so that some of her friends concluded that she was out of her mind. She was emaciated, and had no digestive power at all; there was slight paralysis on one side. Examination disclosed the presence of a scar on the head and inquiry elicited the fact that the woman had been hit on the head. On trephining over the scar, an organized clot or evidence of it was found under the skull, extending in a circle with a diameter of three and one-half inches. Severe hemorrhage resulted from the adhesions of the dura and the calvarium, and only after the dura had been freed completely did the hemorrhage cease.

Before the operation, this patient did have evidences of mental aberration. She could not think much, only to a certain point. She was always in pain. She was much easier after the operation, and she immediately began to be able to follow out ideas. An attack of typhoid fever, during convalescence, was unattended with mental symptoms. She is now becoming strong and complains not at all of the old head-pain.

Dr. J. Chalmers Da Costa said that in the unfortunate cases like those related any plan of treatment that offers even the faintest prospect of success is to be looked for with great eagerness and tried with scrupulous care. While a resident physician in a hospital for the insane, Dr. Da Costa had been deeply impressed with the extraordinary suddenness with which acute insanities were capable of disappearing, and chronic insanities of undergoing mitigation. In a case of chronic mania, with the usual unsystematized delusions, excitement and incoherence, in which carcinoma of the breast developed, extirpation was followed by most remarkable improvement in the mental condition, an improvement lasting for at least four or five months, with abatement of excitement, lessening of insomnia, diminution of incoherence, return of more or less regularity in habits and modes of life. After some months the improvement passed away and the symptoms returned to the old level. In another case, an individual who, while in a condition of acute mania, climbed upon a scaffold, fell, became temporarily unconscious, and after the passing away of the unconsciousness rapidly recovered in the course of two or three days, and was able to return to his home in two or three weeks.

Another man, had the extraordinary experience of introducing a ring over his penis, where it remained for a considerable time with the production of destructive ulceration. The ring was removed with some difficulty and the production of much laceration. At the time of the accident he was in the fiercely excited condition of acute mania,

but very soon after the accident (a few hours) his excitement wonderfully abated, and in a few days he was apparently well.

In spite of cases like these, we would not be justified in advising removal of a breast, the production of cerebral concussion, or the laceration of the penis for the cure of insanity.

In cases in which distinct injuries and distinct operations have apparently been productive of recovery (as the operation of trephining, etc.), great importance is to be attached to what Dr. J. William White has called the effect of the operation *per se*.

There are no doubt cases, and those of Dr. Laplace may be among this number, in which operation has disclosed the presence of a lesion; but the majority of lesions found in insanity are secondary to and not causative of the aberration. Again, there are cases of insanity without any detectable lesion, even by the most careful subsequent pathologic examination. The lesions of the true insanities are in the subtle chemistry of the nerve-cell, and are probably beyond the reach of the microscope or chemic reagent.

There is another objection that has been made by most of the surgical students on this subject. There is an inexcusable lack of careful specification as to the exact nature of a case. For instance, in such a case as Dr. Laplace records, with "a delirium of persecution." In an individual who labors under a simple break-down of the nervous system, who is neurasthenic and presents the mental form of neurasthenia, nothing is more common than ideas of suspicion. The question is: Are the delusions systematized or unsystematized? Is the insanity chronic or acute? What is the relation of delusions to the acts? Are these illusions or hallucinations? Is there insomnia? Does the patient eat? Is he destructive? Has he wicked impulses? What is the state of memory, judgment, reason and will? In other words, what is the exact form of aberration? If a chronic insanity with systematized delusions was cured by the operation, the operation is established, but if the case was acute with unsystematized delusions, the condition is one that may pass away of itself, and apparent cure by operation seems to lose a large amount of its significance. To establish this operation, cases must be produced of terminal dementia and paranoia.

There is another point—that is, the question whether an individual has had an injury of some sort, or even a blow. It seems that the mental effect of injury has to be taken into consideration. There is a mass of well-recorded observations in which very slight blows developed phenomena of a most characteristic nature. It is not at all unusual to have the family attempt to account for the insanity by the history of a previous trauma; but the accounts of these injuries are usually very unsatisfactory, and the various members of the family usually disagree as to when the blow occurred, and where.

Another point—all these acute insanities tend to cure in asylums more strongly than people usually acknowledge. For instance, Spitzka gives a percentage of sixty per cent. of cases of acute mania in the first attack that are usually cured. The cure is not unlikely to take place suddenly, especially if there occurs, for instance, a critical disease, or complications like outbreaks of boils, or in case of an accident or surgical operation. The interruption of the regular progress of the disease by a sudden shock may avert it, but sudden cures are uncertain and untrustworthy. Gradual cure is in favor of permanent cure; sudden cure is in favor of relapse. In thirty per cent. of melancholiacs cure ensues. Trephining, except in unquestioned trauma, or in cases with persistent localized headache or obvious lesion, does not seem to be indicated. It is not possible to see that trephining has any broad field of permanent usefulness in these cases. It has been tried over and over again, and over and again it has disappointed expectations.

Dr. James M. Barton said that he had no experience in trephining for insanity, but he recalled several cases in which after other operations he saw decided mental disease disappear. The most recent one in his experience is that of a patient still in the Philadelphia Hospital. The boy was undoubtedly violently insane and was usually strapped to his bed. A mass of carious bone was removed from the head of the tibia, the disease being probably of tuberculous origin. Much to the astonishment of all connected with the case the mental as well as the physical condition began to improve at once and in a fortnight after the operation there was no suspicion of insanity. Several members of this boy's family, including his mother, are inmates of insane asylums.

Dr. Laplace said in conclusion, that while operation upon such cases as have distinct lesions, or distinct symptoms pointing to a lesion, should be accepted as warrantable today, on the other hand, the other cases that are as yet thought to be attributable to no appreciable lesion are such only because we are unable to recognize the lesion. A lesion must exist and it is the physician's duty to try and find it.

There is no effect without cause; no man or woman becomes insane without a cause. There must be to a certain extent an appreciable disturbance if only we had the microscopic eye sufficient to detect it.

With regard to the large proportion of spontaneous cures of cases of acute insanity, it may be said that while a certain number get well it cannot be known if a case becomes acutely insane how long it will remain so; nor can it be known that it is not the first chapter in an attack of chronic insanity. Underlying the whole problem implied in the influence of the operation *per se* is the question of shock.

A CASE OF VARICELLA GANGRÆNOSA; DIPHTHERIA, RUBEOLA, AND VARICELLA OCCURRING AT THE SAME TIME.

J. P. CROZER GRIFFITH, M.D.

[Read June 10, 1896.]

Charles F., aged twenty-two months, was admitted to the Children's Hospital of Philadelphia on February 25, 1896, suffering with pneumonia. The family history was good, except that the father was just convalescing from pneumonia and looked tuberculous. The child had always been healthy previously. On February 20th he began to have cough and fever. On admission to the hospital he had frequent cough (which appeared to be painful), fever, rapid and labored respiration. Physical examination upon the following day disclosed evidences of croupous pneumonia of the upper part of the left lung. By February 29th the consolidation had increased and all the symptoms of pneumonia were well marked, and there were also decided indications of meningitis. By March 2d the pneumonia was resolving and convalescence was established. The attack had been a severe one and had threatened to be fatal. On March 14th, the child developed slight follicular tonsillitis, and cultures showed the presence of the Klebs-Lœffler bacillus. The general condition was, however, excellent, and the evidences of tonsillitis disappeared rapidly. On March 18th the rash of measles appeared, not preceded by rise of temperature, but five hours later the thermometric reading was 104° F. On March 21st a vesicular eruption appeared, resembling that of varicella (which, like measles, was prevailing in the house at the time), but the vesicles increased rapidly in size until they were of a bullous character and rendered the diagnosis somewhat doubtful. On March 24th and 25th the child was examined by Dr. J. A. Cantrell, who agreed with me that the case was one of varicella gangrænosa. The course of the eruptions from the beginning and for the first ten days, as condensed from notes taken by Dr. Cantrell and myself, was much as follows:

The rash appeared first as a vesicle of varicella. This vesicle rapidly increased in size, because somewhat bullous and then burst, leaving an erythematous area having an appearance as of an emptied blister with the cuticle still preserved and drying over it. Most of these areas were not larger than a split pea or a ten-cent piece in size, but some were as large as a quarter of a dollar. In a few days ulceration began in some part of the erythematous areas, and extended, forming cup-shaped excavated ulcers, oval or rounded in character, with ragged edges and about two lines in depth. From some of these an ichorous pus was discharged, while over others large dark-brown or black-red crusts formed. Two large lesions that were present over the right scapula and two over the posterior surface of the left arm showed a tendency to coalesce. Early in the attack the trunk and arms were principally involved and there were but few lesions on the head and none on the lower extremities. But little inflammatory areola surrounded the ulcers. There was little induration, and the ulceration was, as stated, not deep.

This condition did not continue. New vesicles formed and ulceration extended, and on March 31st it was noted that erythematous spots, rapidly changing to vesicles, but not yet ulcerated, were now forming on the legs, and that ulceration was extending over the scalp, forming superficial lesions, the size of a twenty-five-cent piece, without much crust. Ulcers were present on the neck and face, as well as elsewhere over the body.

On April 2d, twelve days from the first development of the eruption, the child was losing ground rapidly and was very weak. There were a great many large, superficial ulcerations scattered over the body, but most on the scalp and on the back. The inflammatory areola was more marked in places, but not generally so. The child was having sinking attacks, apparently from cardiac failure, with shallow and labored respiration and rales in the chest. Fever of moderate intensity had been present from the onset of the tonsillitis, on March 14th. The child died on April 4th, with symptoms apparently of broncho-pneumonia, although no physical examination was made, owing to the extreme weakness of the little patient.

The only interesting feature of the autopsy was the discovery of diphtheric membrane in the trachea, but not in the larynx.

Varicella gangrænosa is unusual enough to warrant the report of this case. The description of the disease dates from Hutchinson, although it was mentioned long before this. It appears to have been met with chiefly in England. German text-books make but little or no reference to it and it would certainly seem to be rare in our country. I saw a case within a couple of years, in which one spot upon the abdomen developed into a deep slough, but I have never met with so extreme ulceration as in the case now reported. The photograph presented shows the appearance of the child when the disease was approaching its height. Unfortunately the head-covering was not removed, and the ulceration of the scalp does not show.

The eruption in this case exhibited certain peculiarities in which it differed from the rash of gangrenous varicella as ordinarily described. It did not, namely, proceed uniformly onward from a vesicle to an ulcer, but first became a vesicle, then usually a bulla, and then generally burst and dried, as though this were to be the last of it. Then ulceration began in the affected region. Again, although the ulceration was extensive, it did not extend at all deeply—not even through the entire thickness of the skin.

The steady development of new vesicles over so long a period was different from what is observed in an ordinary case of varicella, in which the production of vesicles should have ceased much earlier.

It is probable that this ulcerative process is not characteristic of varicella alone, but may attach itself to other conditions in which great debility exists. In the case reported, however, there seems little reason to doubt that the disease was primarily varicella, as this affection was prevalent in the hospital at the time, and the early appearance of the eruption was entirely that of chicken-pox.

It is interesting to note that the child seems to have suffered at one time from three infectious diseases — diphtheria, rubeola and varicella.

The statement has been made that all post-mortem examinations in cases of varicella gangrænosa have shown the presence of tubercles. No tubercle, however, was found in this case.

I regret that no bacteriologic study of the pus from the ulcers was made. All the officers of the hospital were so overburdened at the time with the unusually large amount of severe sickness prevailing in the institution, that this examination was neglected.

I have not entered at all upon the literature of the subject, as a very full resumé is contained in a recent article by Spivak, and in a discussion of it by Cantrell, in the *Transactions of the Philadelphia County Medical Society*, for 1895.

DISCUSSION.

Dr. F. Woodbury raised the question whether there could have been any possible connection between a remedy given to the child and the profuse bullous eruption on the surface of the skin? He related the case of an adult in which five grains (a comparatively small dose) of potassium iodid would produce a vesicular or bullous eruption resembling varicella. This was tried several times, and within a few hours this eruption would make its appearance. It is conceivable that some children may be similiarly affected by potassium iodid when given even in very small doses. Dr. Woodbury cited the case of a child five years old, with an attack of varicella preceded by a few weeks by an attack of measles and followed by a fatal attack of confluent small-pox.

Dr. J. Abbott Cantrell expressed regret that the name varicella gangrænosa has not been dropped. While the affection resembles varicella and at times develops in the sequence of varicella many instances are reported in which varicella has had nothing to do with the disorder at all.

In some cases an erythematous condition that has been present for some time is followed by a papulo-vesicle or a vesico-pustule, with discharge of the contents, ulceration and the formation of crust.

Dr. Wm. M. Welch said that he had seen many cases of varicella, but never one of varicella gangrænosa. The course of the eruption as described in the case reported is not at all common. Usually the first cutaneous manifestation of varicella is an erythematous condition, consisting of very small red spots. This condition does not continue very long and is rarely seen by the physician, as he is usually called too late. These primary lesions are rapidly converted into vesicles. In the course of a few days the vesicles are found to vary greatly in size, some remaining so small as to be scarcely distinctive, while others increase in size by peripheral extension until they become as large as a silver dime, or even larger. These larger lesions may cover considerable areas and be attended with well-marked ulcerative action, even to the extent of destroying the cutis, as in the lesions of variola, but they do not assume a gangrenous condition. These prominent lesions are quite commonly followed by permanent scars which cannot be distinguished from the pitting of small-pox.

A reference to the literature of varicella shows that varicella gangrænosa is rare. A few cases have been recently reported in Philadelphia, and others have been reported in some other cities, especially in New York. It is generally believed that when this complication is seen it indicates a previously debilitated condition of health. Some writers have explained it by supposing that there is present in individuals in whom it occurs a predisposition to gangrene—whatever that may mean.

Dr. W. B. Atkinson referred to an epidemic which as the representative of the State Board of Health he had declared one of small-pox although the local medical profession believed it to be one of varicella gangrænosa.

A man who had been conducting a small shoe-mending establishment had been pronounced by the physician in the case to be suffering from nothing more than chicken-pox, and was, therefore, allowed to have everybody come into his place. In a short time there was a number of deaths from varicella gangrænosa, three of them in men not less than twenty-two or twenty-three years of age, two of them in one family, without a history of previous bad health; and the disease was continually spreading in every direction. After thorough quarantine of every individual case the epidemic, in a very reasonable time, began to decline and no further trouble occurred.

Dr. J. F. Schamberg said that the fact that the affection described has been the sequel of so many debilitating and infectious diseases brings up the possibility that it is nothing more than the dermic manifestation of some systemic intoxication (other than varicella). Diphtheric paralysis, the result of inflammation of motor nerve-trunks, is a common sequel of diphtheria. Dr. Schamberg thought it not at all impossible that an inflammation of some of the cutaneous nerves could be produced and give rise to bullæ going on to gangrene—just the condition seen in the case reported. Herpes zoster, a disease that results from a spinal ganglionitis or a neuritis presents all grades of trophic change from vesiculation to hemorrhage and gangrene.

Dr. Griffith added, in conclusion, that all of those who had a chance to see the case reported did not have the slightest hesitation in saying it was not small-pox. There was not the inflamed areola, for instance. The experience of those who have seen a good deal of the disease fully justifies the belief that there is a varicella gangrænosa.

All the diseases, however, that run a similar course cannot be called varicella. They need not follow infectious disease; they sometimes occur in pure cases of varicella, developing in children in a bad state of health. They do not need to have been preceded by other infectious diseases. The condition is a distinctly gangrenous one. In bad cases sloughing occurs.

COLLAPSIBLE AND REMOVABLE RUBBER BAGS FOR ALL FORMS OF INTESTINAL APPROXIMATION—A NEW CONTINUOUS DOUBLE-KNOT INTESTINAL SUTURE—A NEW ABDOMINAL RETRACTOR—SELF-RETAINING TENACULA.

A. J. DOWNES, A.M., M.D.

[Presented June 24, 1896.]

About three years ago I conceived the thought that the ideal method of doing intestinal approximation would be end-to-end union by means of a collapsible bobbin, which could be removed from a small slit in the bowel beyond the point of anastomosis. In 1893 Charles Lentz & Sons attempted to make for me such a bobbin out of metal. During the past winter I had them at work upon a bobbin of spring wire, ballooning, with a central constriction, and after use collapsing under pressure and lengthening into a narrow removable cylinder. While perfecting this instrument it occurred to me that an inflatable rubber bag or bobbin of the proper shape would be just the thing. I immediately sent diagrams and dimensions to the Davol Rubber Co., who promptly made me three sample bobbins, alike except in size. One of these I used in my first experiment on the dog. These were very similar to the Barnes uterine dilators. I subsequently improved the bobbin by having a central band one-sixteenth of an inch wide, which thickened and preserved under distention the constricted part between the spherical ends. These bobbins—I have not decided on a better name—which we will call No. 1, are intended strictly for end-to-end anastomosis. They differ only in width. The bobbins proper are two and one-half inches long with a tube extension at one end through which the cavity can be filled. The spherical ends, both alike collapsed, measure from one to two inches, increasing by a quarter of an inch for the different sizes. The diameter of the collapsed bobbins at the constricted portion is three-eighths of an inch less than at the center of the spherical ends.

My next bobbin, which we will call No. 2, differs only from No. 1 in that the spherical ends are of unequal dimensions. Its use is to unite a hollow viscus, such as the stomach, or a section of large bowel with intestine of narrower caliber. Hence it can be used in pylorectomy, gastro-enterostomy, and ileo-colostomy.

These two varieties of bobbin, after completing the anastomosis, are removed from a very narrow slit in the long axis of the bowel beyond the point of union. The incision will vary from one-fourth to less than half an inch. The end of the filling tube of the bobbin is brought out of the bowel through this slit, the bobbin is gently emptied and removed, and the small incision closed by a few sutures.

My next bobbin, No. 3, is for lateral anastomosis. In this the filling tube enters the bag at its constricted portion. The bobbin is introduced into the bowel and a few preliminary sutures taken. It is then filled with fluid and the bowel sutured over it, except where the tube emerges; here the last two or three

stitches are left loose. The bobbin is now emptied, removed, and these few sutures fastened.

It was not until after I had devised this lateral anastomosis-bobbin, removable just before the completion of suturing, that I thought of using the same sort of bobbin for the end-to-end method. I finally had bobbin No. 4 made, which comes in the same sizes and styles as Nos. 1 and 2, except that the filling tube enters the constricted part of the bobbin and it is removed before complete union of the edges of bowel has been effected.

This communication is but a preliminary one. Careful experimentation must yet develop which is the better style of bobbin, the one removable from a small extra incision or not.

While experimenting with these bobbins I improvised a suture which I believe is new, especially in intestinal surgery. It is a continuous double-knot suture. With it we can use the Lembert stitch or not as we elect. The Lembert stitch gives more perfect and broader apposition of serous surface, but I believe at the expense of the caliber of the bowel from subsequent constriction. In my experiments so far on the living animal I have used this continuous suture in the following manner: The needle, threaded with long fine silk, is entered at right angles to the long axis of the bowel, about one-eighth of an inch from its cut edge, passes through the serous, muscular and cellular coats, emerging from the bowel-wall just above the mucosa; it then crosses to the opposite section of the bowel, enters below the mucosa, passes through the cellular, muscular, and serous coats, emerging as near the end as it entered on the other side. This first stitch is tied with a surgeon's knot and the free end of silk cut short. The needle, threaded with the long end, passes through the ends of the bowel about one-twelfth of an inch from the first stitch and in the same manner. The needle is brought across to the opposite side, a double knot taken in the silk just where it had entered for the second stitch, the silk tightened by pulling in the direction of the first stitch, and the knot then set by pulling in the direction of the next stitch. This continues until the circumference of the bowel is united. When the suturing is complete the knots are all on one side, and the stitches placed at right angles to a perfect cross section of the bowel.

When we remove the bobbin before complete suture, the last few stitches will not be placed as true as the others perhaps, and this is an argument against this variety of bobbin, in end-to-end anastomosis. In the method of suturing just described there is less apposition of the serous layers than when the Lembert stitch is used, but there is obtained, what is not claimed for any other method of intestinal suture, almost juxtaposition of the edges of the various coats. The cellular coats meet exactly, the muscular hardly less so, and the serous slightly. It is the simplest thing to take an extra running stitch to obtain more serous apposition.

I am at present conducting a series of experiments on animals, the results of which will be given in a later communication. Those already performed seem to indicate that, with these bobbins and a suture as rapid of application as that described, and as efficient, intestinal approximation will be robbed of many of its difficulties.

The abdominal retractor consists of a piece of wire shaped like a horse-shoe, and provided with retracting blades which project into the abdomen and are prevented from slipping by means of small tips. I have used it in a few abdominal operations, including hysterectomy, appendicitis and cholecystenterostomy.

I also wish to exhibit small self-retaining tenacula with points like the bullet-forceps. They are used for holding the bowel in apposition over the bobbin in place of the preliminary stitch and assist in saving time.

DISCUSSION.

Dr. J. M. Fisher could not agree that end-to-end anastomosis is the ideal mode of uniting intestine when it has been divided in an operation or otherwise, because of the cicatrical contraction that takes place subsequently.

In doing lateral anastomosis the opening can be made as large as one chooses, the result being practically a continuous canal after cicatrization has once taken place. Not only that, but the greater portion of the suturing can be done previous to opening the intestines.

Given a divided bowel the ends are quickly closed by inversion of the edges and continuous Lembert sutures; then bringing the lateral surfaces of the bowel together one is enabled to suture around at least three-fourths of the proposed opening. The opposing lateral surfaces are now incised to any length desired and the suturing completed, thus re-establishing the continuity of the canal. In this way one reduces possible extravasation of feces during the operation to a minimum.

In doing this there is no necessity for the use of either a bobbin or a button.

The abdominal retractor is a most ingenious device, because of the fact that the bar supporting the retracting blades is out of the way. But in certain abdominal work where it is desirable to obtain a view of the pelvic cavity, in hemorrhage, for example, the bleeding point is generally to be looked for at the sides and not along the median line, and this retractor in such a case would be useless. In other cases the intra-abdominal pressure is considerable, so that the introduction of any form of double-bladed retractor would at once allow protrusion of the intestine. The more important part of pelvic surgery, especially in inflammatory cases, is accomplished by touch and not by sight.

Dr. Downes said, in conclusion, that many writers on surgery concede that the ideal method of doing intestinal anastomosis is the end-to-end method, but they have no perfect way of doing it. The only means that has offered any prospect at all is the Murphy button, which has many warm admirers and opposers. Dr. Downes expressed the belief that in the future a bobbin like the one presented will make it possible to unite the bowel end-to-end and leave it with about the same caliber as before section. With the suture described the cellular and muscular layers of the bowel can be brought in exact apposition; with the serous coat sufficiently so.

The current method of effecting lateral anastomosis is pretty certain to result in contraction, no matter how long the slit. The Lembert suture leaves almost a little valve or fold in the bowel which increases from thickening and contraction of lymph where the serous surfaces meet.

PROGRESS IN ORGANOTHERAPY.

AUGUSTUS A. ESHNER, M.D.

[Read June 24, 1896.]

The basis of a great therapeutic advance was established when it was demonstrated by Eiselsberg in 1890 that the clinical syndrome resulting from total extirpation of the thyroid gland—and comprehended in the designation cachexia strumipriva—could be prevented by transplantation of the removed organ in a new situation. Schiff, who, in 1856, had observed that extirpation of the thyroid gland was followed invariably in dogs by death and who was able in 1884 to confirm his earlier observations, found that death could be prevented under these circumstances by grafting a portion of the gland beneath the skin or within the peritoneal cavity. In 1877 Ord pointed out changes in the thyroid gland in cases of myxedema, and in 1882 J. L. Reverdin called attention to similar changes in the sequence of surgical removal of that gland. In 1883 Semon suggested a causal relationship between the loss of thyroid function and the resulting symptoms; and the validity of this proposition was shortly afterward established by an investigation conducted by a special committee of the Clinical Society of London. In 1890, independently of the observation of Eiselsberg, Horsley suggested grafting of sheep's thyroid in the treatment of myxedema, and a little later this suggestion was successfully acted upon by Bettencourt and Serrano. In the same year, Vassale prevented the development in dogs of the phenomena following thyroidectomy by intravenous injection of an extract prepared from the removed gland, and in the following year Murray treated successfully a case of myxedema by hypodermic injection of an extract of thyroid gland. It was soon found that the same good results could be secured by the administration by the mouth of the gland itself or of an extract prepared from it, and the long record of successes that has marked the therapeutic employment of thyroid gland in one form or other elicits the warmest admiration for the scientific acumen and the professional zeal that guided the successive steps by which the underlying principles of organotherapy have been established upon a firm basis.

Within the comparatively short period covered by the discoveries narrated, a vast literature upon the subject has grown up, and the matter has attained an importance the magnitude of which we are even yet scarcely able to realize. Not only has the use of thyroid preparations been extended to the treatment of diseases other than those in which its utility was first demonstrated, but the principle on which this practice is based has been applied to a far wider range of therapeutic purposes, and almost every day brings some new development in this promising field. Already physiologists have succeeded in isolating from the thyroid gland a body designated thyroiodin, which is capable of much of the therapeutic usefulness of the gland itself. The benefits of the new therapy have accrued not only to the physician, but to the surgeon as well; for the latter has learned in the removal of organs physiologically concerned in

some way in the bodily metabolism—and few, if any, organs are not so concerned—to leave behind, if possible, a portion of the glandular structure.

Of the results obtained in the treatment of myxedema and cretinism with thyroid preparations it is scarcely necessary to speak, so uniformly successful have these results been. To insure their permanence, however, the treatment must be persisted in, though occasional intermissions are attended with good effects rather than otherwise. By organotherapy there is supplied to the body a substance or substances essential to metabolic equilibrium, but wanting through glandular deficiency. The effect ceases with the withdrawal of the agent with whose aid that equilibrium is artificially reëstablished. It must at the same time not be forgotten that the glandular preparations thus used are capable of harm when employed in excess, and appropriate precautions should in all instances be observed to prevent deleterious effects.

In view of the pronounced effects of thyroid therapy upon the cutaneous and subcutaneous and allied structures, as observed in the treatment of myxedema and cretinism, it was an easy step to the employment of the same measure in the treatment of other diseases in which the skin is affected. The results secured in the treatment of obesity with thyroid preparations have been scarcely less successful than those in myxedema and cretinism and do not require extended consideration.

In 1893 Bramwell reported before the British Medical Association cases of psoriasis treated with thyroid extract, with results "at once surprising and gratifying." Others have had a similar experience. A successful result has also been reported in a case of syphilitic psoriasis in which other measures had already been employed without relief.[1]

Believing that the special susceptibility to tuberculosis manifested by cases of myxedema might be due to the absence of thyroid function Bramwell[2] was led to employ thyroid extract in the treatment of lupus, and the results were so favorable as to suggest the applicability of the same method in the treatment of other forms of tuberculosis. Thyroid extract has further been used in the treatment of eczema, dermatitis exfoliativa, rosacea, universal alopecia, pityriasis rubra, ichthyosis, scleroderma and xeroderma, with varying, though in general not disappointing, results. It has also been suggested that the remedy might prove useful in the treatment of leprosy, erysipelas and carcinoma. In the case of a young woman presenting an hypertrophied scar of the face White[3] observed reduction in the size of the disfiguring cicatrix in the sequence of thyroid therapy in conjunction with topical applications of collodion, after other measures had failed.

Bramwell[4] has reported excellent results from the employment of thyroid extract in the treatment of tetany, and from the association of this disorder in infants with rickets and laryngismus stridulus he has suggested the possible efficacy of similar treatment also in these latter conditions. Perhaps, too, the same procedure might be effective in other spasmodic and convulsive disorders. Cases of tetany successfully treated with thyroid extract have also been reported by other observers.

Thyroid extract has been used in the treatment of cases of rachitis, associated with anemia, with resulting general improvement and gain in weight, but without appreciable changes in the bones.[5]

The improvement in the mental state noted in cases of myxedema and cretinism subjected to thyroid therapy naturally led to the employment of thyroid preparations in the treatment of insanity. According to Bruce[6], the remedy is especially useful in cases of insanity at the adolescent, climacteric and puerperal periods; when recovery is slow; and in cases of long standing with a tendency to dementia. It is contra-indicated in cases of mania attended with acute excitement, rapid loss of

1 Gordon: *British Medical Journal*, No. 1726, p. 186.

2 *British Medical Journal*, No. 1737, p. 786.

3 *University Medical Magazine*, August, 1895, p. 812.

4 *British Medical Journal*, No. 1796, p. 1196.

5 Heubner: *Wiener Klinische Wochenschrift*, May 26, 1896, p. 452.

6 *Journal of Nervous and Mental Science*, xli, January and October, 1895.

weight and danger of exhaustion from malassimilation of food.

Reports of the results secured in the treatment of exophthalmic goiter with preparations of thyroid gland have been varied and conflicting — improvement being noted in some cases, aggravation in others. In the few cases in which I have adopted this plan, the resulting improvement was not greater than I have observed following the administration of strophanthus or the salicylates. Successful results have also been reported from similar treatment in cases of simple goiter, especially of the parenchymatous variety. Relief likewise was afforded in a case of exophthalmic goiter in which thymus gland was administered in mistake for thyroid.[7] In a case of exophthalmic goiter, complicated by scleroderma, the symptoms of both morbid conditions were relieved by treatment with thyroid gland.[8]

Having observed in two cases the development of osteo-arthritis in the sequence of removal of the uterine appendages, Macalister[9] learned, upon inquiry in a number of other cases, that some disorder of uterine or ovarian function had preceded the articular disease. As a result of these and other observations, he formulated the hypothesis that the glandular structures of the body elaborate substances that exert a controlling influence over the growth of individual tissues, and that an excess or deficiency of any tissue-element is attributable to the absence or perversion of the secretion that physiologically controls the growth of that particular constituent. In accordance with these views, he employed thymus gland in the treatment of a case of pseudo-muscular hypertrophy, upon the assumption that the symptoms of this disease are dependent upon the removal of an influence inhibiting the growth of the fibrous parts of the muscles, in consequence of premature atrophy of that gland; and in a case of lymphadenoma in an old man, he administered a mixture of red and yellow bone-marrow in equal parts. Lépine[10] has reported two cases of muscular dystrophy in which dynamic improvement, without other change, followed thyroid treatment.

Looking upon the red marrow of bone as the chief agent in promoting the development of red blood-corpuscles, Mann[11] was led, in 1894, to use a glycerin extract of marrow obtained from the long bones of calves, in the treatment of anemia; and in a series of cases of varying kind and origin, encouraging results were secured. A little later, Fraser[12] reported a case of pernicious anemia in which remarkably satisfactory results followed the employment of bone-marrow, in conjunction with arsenic, iron and salol. Since this time, a considerable number of cases of grave anemia of varied type have been reported, in which bone-marrow was used, and the results, while in some degree conflicting, are on the whole encouraging and justify the further employment of the remedy. From the evidence that has accumulated, there can be no doubt that bone-marrow taken by the mouth is capable of favorably influencing the state of the blood, and as anemia arises from a multiplicity of causes, it should not occasion surprise that any remedy should fail in some cases.

In 1894, Bigger[13] reported a case of leukemia in which recovery followed the therapeutic administration of bone-marrow, after arsenic and iron had been employed without success. Lawrie[14] has also reported a case of leukemia successfully treated with bone-marrow. Bone-marrow, in conjunction with splenic tissue, has also been successfully employed in the treatment of malarial cachexia.[15]

Having observed spontaneous disappearance of the symptoms of exophthalmic goiter in a case complicated by the development of splenitis—probably of traumatic origin—and the formation of a splenic abscess, subsequently evacuated, Wood[16] was led to employ hypo-

7 Owen: *British Medical Journal*, No. 1781, p. 361.

8 Grünfeld: *Wiener Medizinische Wochenschrift*, 1896, No. 20, p. 307.

9 *British Medical Journal*, No. 1684, p. 729.

10 *Lyon Medical*, 1896, No. 19, p. 35.

11 *Lancet*, No. 3680, p. 599.

12 *British Medical Journal*, No. 1774, p. 1172.

13 *Lancet*, No. 3708, p. 682.

14 *British Medical Journal*, No. 1770, p. 1238.

15 Critzmann: *Presse Médicale*, 1895, No. 68, p. 507.

16 *University Medical Magazine*, February, 1895, p. 313.

dermically a glycerin extract of beef-spleen in the treatment of a later case of exophthalmic goiter, with results that were in every way gratifying.

For a long time, physiologists have realized the fatality of total extirpation of the pancreas, and pathologists had early observed changes in the pancreas in fatal cases of diabetes examined after death. It is, however, only within recent years that experimental removal of the pancreas has succeeded without immediate death; and under these circumstances glycosuria, polyuria and wasting invariably developed. Here, again, it was found that if a portion of the gland were permitted to remain or were grafted in a new situation, the symptoms failed to appear. Pancreatic preparations have been employed in the treatment of a number of cases of diabetes in the hands of different investigators, with resulting improvement in some cases.

It is the consensus of opinion that the symptoms of Addison's disease are dependent upon changes in the suprarenal bodies; and in conformity with this view a number of clinicians have employed suprarenal extracts in the treatment of that disease.[17] Oliver, who, in conjunction with Schäfer, has made a study of the physiologic action of suprarenal extract, recommends its use also in cases attended with loss of vasomotor tone, in exophthalmic goiter, in cyclic albuminuria, in diabetes insipidus and diabetes mellitus, and in cases of capillary hemorrhage. In cases of anemia thus treated, he has observed a rapid rise in the percentage of hemoglobin.[18]

In a case under my observation, presenting Addisonian symptoms, no appreciable benefit followed the use of such an extract. Post-mortem examination, however, failed to disclose distinctive changes in the suprarenal gland.

It has been shown experimentally that the symptoms resulting from removal of the pituitary body—lowering of temperature, anorexia, lassitude, convulsive movements and dyspnea—can be prevented by injections of pituitary extract. In some cases of akromegaly relief has followed employment of a similar extract.[19] In the discussion following the report of a case of akromegaly that it was my privilege to make to this Society in 1895[20] I took occasion to refer to the possible utility of a preparation of the pituitary body in the treatment of that disorder.[21] In accordance with this thought Mess. Armour and Co., of Chicago, at my request kindly prepared for me such an extract (of which one part of the desiccated product represented seven parts of crude pituitary body), of which I began the administration of one grain thrice daily, but the patient did not remain long enough under observation and no therapeutic effect was noted. At about the same period or a little later Marinesco[22] reported to the Société Médicale des Hopitaux three cases of akromegaly treated with pituitary extract in which symptomatic improvement resulted. Bramwell[23] and Murray[24] have employed thyroid extract in the treatment of akromegaly, but without pronounced effect.

Actuated by the results secured in the treatment of goiter with thyroid extract Reinert[25] was led to employ the prostate gland of steers in the treatment of four cases of prostatic hypertrophy, in two of which reduction in the size of the enlarged gland was noted. If these observations be correct the validity of the fact cannot be negatived by *a priori* considerations; although one would naturally look for therapeutic effects from prostatic administration in the presence of symptoms attributable to loss of function of the prostate in consequence of surgical removal or of disease. A parallel statement may be made concerning the employment of testicular extracts. At the same time it is not necessary to deny that from their nature

17 Shoemaker and Wood: *University Medical Magazine*, February, 1895, p. 309. Rolleston: *British Medical Journal*, April 16, 1895, p. 745. Jones: *British Medical Journal*, August 24, 1895, p. 482. Oliver: *British Medical Journal*, August 31, 1895, p. 561. Sansom: *British Medical Journal*, November 16, 1895, p. 1235. Osler: *International Medical Magazine*, February, 1896, p. 3.

18 *British Medical Journal*, September 14, 1895, p. 683. *Pulse-Gauging*, London, 1895.

19 Caton: *Lancet*, February 9, 1895, p. 349.

20 *Transactions of the Philadelphia County Medical Society*, 1895, xvi, p. 308.

21 *Loc. Cit.*, p. 314.

22 *Mercredi Médical*, 1895, No. 46, p. 550.

23 *Atlas of Clinical Medicine*, vol. ii, p. 3.

24 *British Medical Journal*, February 9, 1895, p. 293.

25 *Centralblatt fur die Krankheiten der Harn- und Sexual-Organe*, B. vi, H. 8, p. 393.

all organic extracts may possess stimulating properties.

The most recent development in the domain of organotherapy consists in the employment of preparations of the ovaries of animals in the treatment of the symptoms resulting from removal of the functional influence of the ovaries in women either at the natural menopause or at that induced artificially by surgical intervention or by disease-processes. Observations upon these lines seem to have been made almost simultaneously and independently by Mainzer and by Chrobak in conjunction with Knauer. To the former belongs the credit of priority of announcement[26] who reports a case in which relief of symptoms followed use of an ovarian extract. Chrobak[27] had independently conceived the idea that the distressing symptoms so often observed after ovariectomy could be prevented by permitting to remain a portion of ovarian tissue and that they could be relieved when present either by ovarian grafting or by internal administration of some preparation of the ovary. Acting upon this thought he has of late years in operations upon the uterus and ovaries made a practice whenever possible of leaving behind a portion of ovarian tissue. He has, besides, during the past year employed an ovarian extract in a number of cases in which the ovaries had previously been removed and in one with normal genitalia in which profound climacteric symptoms were present. The results, so far as they could be analyzed, were satisfactory and encouraging. The experiments of Knauer,[28] undertaken at the suggestion of Chrobak, show not only that the ovaries are susceptible of successful transplantation, but also that they are capable of functional activity in their new situation.

In the foregoing account I have not attempted to enter upon an exhaustive consideration of the whole subject of organotherapy, but have endeavored merely to illuminate some of its more practical aspects. There is much yet to learn, perhaps not a little also to unlearn, but a good deal of what has been accomplished will permanently endure; while the outlook for the future is hopeful and encouraging.

[26] *Münchener Medizinische Wochenschrift*, 1896, No. 12, p. 188.

[27] *Centralblatt für Gynäkologie*, 1896, No. 20, p. 521.

[28] *Centralblatt für Gynäkologie*, 1896, No. 20, p. 524.

DISCUSSION.

Dr. B. Meade Bolton maintained that the whole subject should be divided sharply into two headings: the use of extracts like the thyroid, thymus, and adrenal on the one hand, and the use of those like the prostatic, the ovarian and the testicular on the other. A moment's consideration will show that in the latter case an attempt is made either to stimulate or to substitute a physiologic action. It is a different matter whether an attempt is made to stimulate or to give a tonic for the sexual organs or whether the object is to try to neutralize the toxic effect due to the insufficient action of the thyroid, thymus or adrenals. The use of thymus and thyroid extracts is really to neutralize a toxic condition of the system. The animal or the person suffers really from a sort of intoxication when these glands are removed and the addition of a small part of the glands or an extract of these glands simply neutralizes the poison. In other words the system is constantly accumulating effete products of the metabolism of the cells, which normally are neutralized by these various glands. By substituting then some of the extract from another animal this poison is neutralized which would be neutralized in a normal animal by the normal gland.

On the other hand, the use of the prostate and of the ovaries is intended to stimulate already existing physiologic functions. The activity of thyroid and thymus extracts is shown in a most interesting way by the work of Brieger, Kitasato and Wassermann, who cultivated various organisms in the extracts of these glands and found that the organisms had lost their toxic power, although they could still be used for inoculating animals. Dr. Bolton added that definite results have been obtained from the therapeutic use of thyroid and thymus extracts. In the use of the other extracts from the ovaries, testicles, etc., it is possible that the element of hypnotism or suggestion has not always been carefully eliminated.

Dr. S. Solis-Cohen said that he had made many clinical observations on the use of ani-

mal extracts, some of which have from time to time been published. In addition to the distinction between the use of preparations of the sexual glands and of other glands, a further important distinction is to be made, which may be expressed in classification by the terms *extrinsic* and *intrinsic*; perhaps *homologous heterologous* would be better. When a substance prepared from a normal gland is used to compensate therapeutically for a deficiency in the patient an intrinsic or homologous action can be spoken of. By art is substituted the animal secretion or extract for that which the patient's gland should naturally supply, this being deficient from disease; as in the treatment of myxedema by thyroid extract. When, however, thyroid extract, for example, is used in the treatment of psoriasis and other diseases not dependent, so far as we know, upon lack of thyroid function the animal preparation is used just as an agent derived from the mineral or vegetable kingdom would be used, because of certain anatomic, chemic or physiologic effects which it is supposed to be capable of producing and which are not related directly with the function of the organ from which it is derived. This may be termed extrinsic or heterologous action. The use of the heterologous action of animal extracts is largely governed by empiricism; this may be based on laboratory investigation or be purely clinical. But clinical empiricism is of value also, and indeed unless laboratory experimentation were finally submitted to the test of clinical experimentation its results would be valueless from the standpoint of therapeutics.

Dr. Cohen has found preparations of the thyroid gland useful in the treatment of disorders of the vaso-motor system. This is empiric, partly, but nevertheless has a physiologic basis. If one reflects for a moment upon the great number of vaso-motor phenomena which are met with in diseases such as exophthalmic goiter, myxedema, akromegaly, in which the thyroid gland is abnormal, either structurally or functionally, he can hardly avoid the conclusion that this organ is in some manner related with the taxis of the cardio-vascular function. Dr. Cohen has gradually been led to divide vaso-motor disorders (vaso-motor ataxia, as he likes to call them) into two great classes, which can be roughly characterized by the reaction of the patient's temperature. Certain patients complaining of vaso-motor disorders will exhibit an undue susceptibility to cold; they will be miserable during winter and comfortable during summer; in these patients vascular contraction is the predominant symptom. Other patients will exhibit exactly opposite tendencies; the vessels become unduly relaxed with but slight elevation of external temperature. In those patients who are peculiarly susceptible to cold the use of thyroid extract will usually be found to be beneficial; while on the other hand, in those patients who are unduly susceptible to heat, the use of thyroid extract often aggravates the symptoms. This is well marked in certain cases of exophthalmic goiter in which affection the tendency to relaxation of the vessels usually predominates. In these cases use of the extract increases the tendency already existing to overaction of the heart, to sweating, to polyuria, to subjective sensations of heat and the like. In some cases of exophthalmic goiter, however, thyroid therapy has been reported by competent observers to have done good. But it is especially in those cases varied in symptomatology, from the excessive vascular constriction of Raynaud's disease to the occasional attack of spasmodic migraine, in which before the introduction of thyroid extract one would have been likely to use nitro-glycerin as a therapeutic agent, that thyroid extract administered in moderate doses is frequently palliative and sometimes actually curative.

Dr. Cohen cited the case of a man who had been for years subject to attacks of blindness associated with vertigo, with pallor of the face, followed by intense headache and accompanied with a feeling of numbness and almost paralysis of one side of the body. Curiously this man's thyroid gland was enlarged and there was a history of his mother having had a goiter. Under treatment, first, with nitro-glycerin, in order to determine his therapeutic peculiarities, and afterwards with thyroid extract, this man has so far recovered that the intervals between the attacks have been lengthened from weeks to months; and the severity greatly lessened. They are now brought on only by some indiscretion in diet, while formerly they occurred in response to various exciting causes, and are immediately relieved or even cut short by return to treatment. Thyroid extract has likewise proved useful in obesity and as a diuretic in various conditions.

Dr. Cohen's experience with the use of thymus extract in exophthalmic goiter has been so far satisfactory that in some five or six cases in which it has been used improvement in symptoms has taken place. He was, however, extremely reluctant to draw positive conclusions from early results observed in exophthalmic goiter in sequence to any kind of treatment, because symptoms so often recede spontaneously under the most divergent methods of treatment. Thus while thyroid extract is often harmful in this disease, a case was cited which improved so much under thyroid treatment that it was impossible to keep the patient under observation. In four cases of simple goiter treated with thyroid extract no improvement was observed.

Dr. Cohen had not seen any permanent good results from the use of pancreatic extract in the treatment of diabetes, although he had observed remarkable temporary improvement; especially in a severe case in a young man in which, under the use of pancreatic extract, the

patient survived some two years longer than was expected and was made quite comfortable during at least the first three months of this treatment. If there were any sure means of recognizing during life those cases in which the diabetic complexus of symptoms is due to disease of the pancreas there would be a clearer indication for the use of the preparation. The use of preparations of the supra-renal capsule will probably prove of service in cases other than of Addison's disease. These extracts seem to exert an effect on the heart and vessels, possibly through the nervous centers, opposite to that of the thyroid extract, namely to heighten blood-pressure. They could be used in such conditions as have in the past been treated with belladonna, picrotoxin and the like. Dr. Cohen's experience with their use in this class of cases has not yet been sufficiently extended to warrant the utterance of a positive opinion, but is in some respects, favorable. He awaited a suitable case and opportunity to test them in exophthalmic goiter.

Dr. J. F. Schamberg said that the magnificent results obtained in the treatment of myxedema with thyroid extract suggested a field for the use of this remedy in various skin-affections attended with great structural changes, particularly in scleroderma. In this disorder, however, the results have been disappointing.

With reference to psoriasis, Bramwell's original paper was too sanguine and has not been substantiated by subsequent observation. The results of the use of thyroid extract in psoriasis have been, to say the least, erratic and disappointing. In some cases in which it has been used the results seem to have been rapid and brilliant, but it must be remembered that psoriasis tends, at times, to spontaneous recovery and that it requires large statistics to prove that any given remedy produces given results. Thyroid extract is certainly inferior to arsenic and the iodids in psoriasis. Furthermore, its use is not unattended with risk. In a case of mycosis fungoides under the care of Dr. Duhring at the University Hospital, it produced an erysipelatous eruption with high fever and tachycardia and almost determined a fatal result.

In regard to the other affections named, eczema, alopecia areata, etc., it is exceedingly dangerous to ascribe curative virtues to a new remedy, after limited observation, considering the frequency of spontaneous improvement in these diseases.

Dr. L. Jurist related a case of what appeared to be a mediastinal tumor in which thyroid extract was employed with brilliant results so far as the patient's freedom from anasarca temporarily was concerned. The circulatory stimulation, however, became subsequently excessive and the remedy had to be withheld.

Dr. Eshner said that it is important to bear in mind that every organ in the body probably plays some part in the general metabolism, and it has been well pointed out within recent times that not only do the so-called ductless glands, which have no external secretion, possess an internal secretion, but also those organs that do have an external secretion, e. g. the pancreas, which, besides its digestive secretion, is also in some way capable of influencing the sugars of the body in such a way that they do not appear in the urine as glucose. Failure in this function results in glycosuria. The full usefulness and the limitations of organotherapy have not yet been settled, and the subject is still in process of agitation. If the vascular effects described by Dr. Cohen were determined by clinical experience, the observations may be looked upon as a happy confirmation of what physiologists have determined in the laboratory. It has been shown experimentally that thyroid extract causes dilatation of the vessels, while, on the other hand the adrenal extract slows the heart and causes contraction of the vessels.

Dr. Cohen stated that his observations had been purely clinical. To the list of affections in which thyroid extract has seemed to be useful, he added rheumatoid arthritis. It is well known that disease of the thyroid gland has been associated in some cases with rheumatoid arthritis; this fact suggested the use of thyroid preparations in the treatment of the latter affection. The observations have not gone far enough, however, to determine its value definitely.

SEVERE STOMATITIS FOLLOWING THE ADMINISTRATION OF POTASSIUM IODID.

JAY F. SCHAMBERG, A.B., M.D.

[Read June 24, 1896.]

The following case is deemed worthy of report on account of the important diagnostic and therapeutic problems which it presents for solution.

A. M., a female, fifty-four years of age, presented herself at the skin-department of the Polyclinic Hospital, on the twenty-sixth of May, 1896, with multiple gummata of the tongue and a tubercular syphiloderm of the face. She was ordered potassium iodid in five-grain doses, to be taken thrice daily. The patient took the first dose on the evening of the same day. On the following morning, she experienced symptoms of conjunctival irritation, and at the same time some tenderness of the gums. Six days later, she again presented herself at the clinic. At this time the following phenomena were observed: The conjunctivæ were intensely injected up to the corneal margin; there was a serous nasal discharge; the patient complained of frontal and malar pain; the saliva dribbled from the mouth; the breath was offensive; the gums were eroded, spongy and bleeding; there was distinct ulceration along the dento-gingival border, especially of the canine teeth. The iodid was continued in the same dose, and the patient was seen three days later. The stomatitis was somewhat improved; the conjunctivitis remained unchanged; the lesions upon the tongue and face were undergoing rapid involution. The dose of the drug was then reduced to two grains thrice daily, and an eye-lotion and mouth-wash ordered. From this time on, both the mouth and the eyes improved. At the present time, the conjunctivitis is well. The gums are still slightly congested and denuded of epithelium, but the fetor and the salivation have disappeared. The prescription was compounded at the pharmacy of the hospital, and the apothecary was emphatic in his asseverations concerning its accuracy. The patient was questioned as to the antecedent ingestion of mercury. She is a woman of fair intelligence and apparently has a good memory. She stated that she had taken no drugs for two or three years, with the exception of one bottle of a patent medicine, which she took about six months ago. She was subjected to repeated interrogation, but adhered to this statement. This information is essential to a proper consideration of the case, inasmuch as the stomatitis had all the features of the classic "stomatitis mercurialis." In fact, those who observed the case were inclined to the belief that the potassium iodid had simply set free hitherto insoluble mercurials which had been deposited in the various tissues. This eliminative action of the iodids has, however, been recently challenged.

Dr. J. William White, in his admirable article on the treatment of syphilis, in Morrow's "System of Genito-Urinary Diseases," says, "the observations of Melsens and Guillot, long quoted in support of this view (namely, that the iodids do good in syphilis by eliminat-

ing the mercury), have recently been contradicted by Suchoff, who asserts that the administration of the iodids really retards the elimination of mercury." Whether this be true or not, it matters not. The history of the case under discussion strongly militates against the view that the stomatitis is due to mercury eliminated by the iodid. Schuster has proved that all mercury is entirely removed from the system six months after its ingestion, even though a long course be taken. The patient took but one bottle of an unknown mixture six months ago, and prior to that time nothing for two years. It is extremely improbable that sufficient mercury could have been in the system to have produced the result narrated. We may, therefore, reasonably exclude mercury as an etiologic factor.

In studying the subject of iodism, one is struck by the fact that the iodids seem to be capable of setting up inflammations of almost any mucous membrane. Conjunctivitis, rhinitis, pharyngitis, laryngitis with edema, and bronchitis may all be produced by their administration. From *a priori* reasoning, therefore, it would not seem at all strange if the same drug were capable of producing a stomatitis. Lauder Brunton, Rilliet and other authors, indeed, do mention salivation among the rarer toxic effects of the iodids. It is to be deplored that the terms salivation and stomatitis are at the present time used indiscriminately. Salivation means merely an increased flow of saliva, a sialorrhea. We take it that the authors quoted refer merely to this functional change.

Bumstead and Taylor state that "salivation sometimes occurs after the use of the iodids, but is never so severe as that occasioned by mercury, nor is it ever attended by ulceration like the latter."

Fournier, in his incomparable work, "*Le Traitement de la Syphilis*," says, "the iodids excite at times a certain degree of salivation. This salivation does not at all resemble that of mercury. It has neither the abundance, the odor nor the inflammatory phenomena. It is a cold sialorrhea, if I may use the expression, with absolute integrity of the mouth. It is at most comparable to the sialorrhea of pregnancy."

This is certainly a most formidable array of opinion against the supposition that the iodids may produce a stomatitis such as the one in question. Nevertheless, Kaposi, one of the foremost German syphilographers, says that "the iodids may, in rare cases, cause a gingivitis and stomatitis." With these facts in mind, the following propositions are tentatively suggested:—

Sialorrhea is the first stage of mercurial stomatitis.

The iodids are freely eliminated by the salivary glands.

In some cases they may produce sialorrhea or salivation.

In extremely susceptible individuals, gingivitis and stomatitis have been produced (Kaposi).

In the case under consideration, the iodid idiosyncrasy was marked.

The possibility of mercurial stomatitis has been firmly eliminated.

This leads us to the expression of the opinion that the stomatitis reported was due to the administration of potassium iodid. This view is confirmed by the coincident appearance of the stomatitis with the other symptoms of iodism. After five grains of the drug had been taken, conjunctival irritation and tenderness of the gums manifested themselves synchronously. If potassium iodid can produce a sialorrhea, it may, in more susceptible cases, produce a stomatitis which, in still more susceptible individuals, may go on to ulceration. In other words, we may have as a result of the administration of potassium iodid a stomatitis which differs in no respect from *stomatitis mercurialis*.

DISCUSSION.

Dr. S. Solis-Cohen said the opinion is now held by many that, in mercurial stomatitis, the mercury is not alone responsible for the ulcerative phenomena, but that breach of the integrity of the tissues through the mercurial irritation, especially in patients who neglect the hygiene of the teeth and mouth, permits certain microbes found in the mouth, and harmless to healthy tissues, to set up ulcerative processes in the inflamed mucous membrane. If this be true, and the view is held by some of the foremost syphilographers of France, the fact will help to explain Dr. Schamberg's case. Iodism in a susceptible subject caused irritation, perhaps inflammation, of the oral mucous membranes; the subsequent phenomena were septic.

Dr. H. Langrehr related that some twenty years ago he observed a case of quite intense salivation, caused by sirup of iron iodid, in which there was no suspicion of previous mercurial treatment.

Dr. L. Jurist said that there can be no doubt that the intense irritation, due to the elimination of a small amount of potassium iodid in a susceptible subject, may be responsible for such symptoms as those described. The condition of the mouth prior to the administration of mercury or iodid for any reason will largely influence the occurrence of stomatitis. It is well known that some patients cannot take small doses of iodid in any form, while others can take large quantities with impunity.

Dr. Schamberg added that positive observations are more valuable than negative ones, and most of the statements made by the syphilographers have been negative in character. Stomatitis following the administration of sirup of iron iodid must be attributed to an idiosyncrasy, inasmuch as this preparation contains comparatively little iodid. Indeed, it is valuable in syphilis chiefly as a tonic, and not as a specific. If such a small amount of iodid can produce a stomatitis, how much more likely it is for a larger amount in the form of potassium iodid to produce a similar condition.

ELECTRICITY IN GYNECOLOGY AT THE HOWARD HOSPITAL.

G. BETTON MASSEY, M.D.

[Read September 9, 1896.]

At a time when the attention of practitioners specially interested in the diseases of women is so largely occupied with the mere operative treatment of these affections, it is worthy of note that through the liberality of the Managers of the Howard Hospital in this city, a complete equipment has been provided for the application of electricity to suitable cases of these affections, affording an opportunity for the worthy poor to obtain the benefit of this remedial agency in the dispensary and wards, as well as pay-patients in the private rooms. The result has been a distinct broadening of the scientific and philanthropic work of an institution that is well represented by able practitioners in the other departments of medical work.

By way of preface to this brief review of it, I may say that the electro-therapeutic work at this institution has not been conducted with any hostility to other modes of treatment commonly employed in similar cases, though a special effort has been made to show its comparative value in certain affections, in the interests of both science and humanity; and particularly, it should be said that no pretension has been made that it is adapted to the cure of all of the diseases of women. It is but a helpmate to the other remedial agencies of a well-equipped general hospital or important medical center, the youngest helpmate in fact, and dealing as it does with a force that is the latest and most valuable acquisition of mankind, has demanded the unremitting attention of special workers, that knowledge and skill already acquired may be usefully applied and new scientific facts discovered. That its practice trenches upon the alleged field of other workers is a mere assumption on their part. There are no preserves in the practice of medicine that can successfully resist the inroads of a better form of treatment, and no truly scientific workers will set up any other standard of action than the best interests of the patients confided to their care.

The cases accepted as appropriately treated by electricity in this department have been strictly gynecologic, the majority presenting evidences of chronic inflammations, exudations or hypertrophies of the pelvic organs, neoplasms, pathologic displacements or disturbed innervation, including certain neuroses with pelvic manifestations and post-operative painful affections.

During the eight years that this work has been conducted a considerable experience has been accumulated, leading to conclusions that may be of general value when summed up in concise form.

Fibroid Tumors.—Thirty cases of fibroid tumors of the uterus have been under treatment for varying periods, including several now in attendance. The results accomplished have been reasonably good, considering the roving nature and imperfect intelligence of the majority of clinic patients, many of whom fail to attend regularly, or stop altogether after their discomfort has been removed. A submucous tumor in one case was expelled from the uterus by electrically induced contractions. Three tumors disappeared almost en-

tirely by absorption, one of them so large as to fill the abdominal cavity below the navel. In eight cases there was symptomatic cure and great reduction in size. In another eight cases symptomatic cure and slight reduction in size were the result. In two cases there was cure of the symptoms but no reduction in size. Four cases are still under treatment with a favorable outlook. In one case only was there no improvement. Three cases failed to continue the treatment after so slight a trial of the method as to leave the possibility of benefit undetermined.

Including the symptomatic cures of tumors that were arrested in growth, with or without reduction in size, the list shows that twenty-two cases were practically successful. As seven are still under treatment or were lost sight of after insufficient treatment, we have twenty-two practical successes, contrasted with but one known failure to improve, or about ninety-five and one-half per cent. of successes and four and one-half per cent. of failures.

No bad results whatever attended the treatment of any one of the cases, and practically no discomfort attended or followed any of the applications. Two cases were, however, operated upon by colleagues after the cessation of treatment, one of these, tabulated as the only case receiving no improvement after a proper length of treatment, dying under the knife, and the other, tabulated as receiving insufficient treatment, being successfully operated upon by Dr. Hamill.

An impartial review of these cases cannot do otherwise than convince the unprejudiced that the electric control of the nutrition and forced absorption of fibroid tumors may be brought about in a considerable proportion of cases, and that a surgeon is not warranted in subjecting a case of this nature to the dangers and uncertain consequences of an operation for removal until the possibilities of the Apostoli treatment have been fully tested. The conditions which contra-indicate the Apostoli method have now been fully defined, and include purulent formations in the tumor or its neighborhood, and cystic degeneration of the tumor itself. The existence of cystic degeneration is easily ascertained by palpation, and purulent inflammations of the adnexa are now also capable of being predicted with great certainty by means of a special diagnostic response to electric treatment, which has been pointed out by Apostoli. It has been ascertained that if the faradic current fails to relieve the pain complained of temporarily, and if the pain is aggravated by an intra-uterine galvanic application, the existence of a subacute purulent inflammation may be predicted with confidence. In such cases electricity intelligently applied becomes of great service to surgery in demonstrating the necessity for an early and thorough operation for the removal of a tumor that is unadapted to any other treatment. The one case mentioned as not relieved in any way by electric treatment gave distinct responses of this nature, intense cramp-like pain being provoked by intra-uterine treatment. At the operation an ovarian abscess was found on one side and a cystic ovary on the other, both presumably of gonorrheal origin.

The treatment of some of these cases was tedious, and in the end some of the twenty-two successes have bunches of harmless fibrous tissue still remaining in the uterus, but each one of these patients presents a final result infinitely superior to the results attained in successful cases of hysterectomy for fibroid tumors in the following particulars, aside from the fact that their lives have not been placed in jeopardy: they still possess their ovaries and the womanly functions dependent on these organs; they have no abdominal scar, with existing or threatened hernia at its site to render their condition about as miserable as before seeking relief, and they have had no stump left in the abdomen to become attached to the intestines to produce after-pains and constipation.

Chronic Metritis.—Of a large number of cases of chronic metritis, or hyperplasia consequent upon catarrhal inflammation of the uterine cavity, sixteen cases are recorded in the hospital-books as receiving electric treatment. Of these as many as thirteen were cured, two improved, and one not improved.

The cured cases reported complete relief from pain and bearing-down sensations; the uterus was found to be shrunken to near or quite its normal size; and some of them reported subsequent pregnancies.

Chronic Perimetric Inflammation.—Fourteen cases of chronic perimetric inflammation, that is cases in which the uterus was enlarged, fixed, and surrounded by inflamed and adherent tubes and ovaries, were under treatment, six of which were cured six improved, and two not improved.

Chronic Ovaritis. — Of the same general nature were four cases of ovaritis, two of which were cured and two improved.

These three groups of cases constitute a very important portion of the affections for which parous women seek medical help, and when it is remembered that a common pathologic factor enters into them all—simply an arrested resolution following a microbic invasion, or germ-phagocytic contest, it is clearly evident that a line of treatment is indicated for all that will arouse dormant nutritive energies and assist the local tissue-scavengers, the phagocytes and lymphatics, in re-conquering the territories in which they have been so far defeated. Embryonal cell-proliferation and tissue-degradation are but other names for the debris and disorganization of this drawn battle, and it behooves us to step in and assist in reorganization rather than lop off these territories in hasty despair.

Post-Operative Pain. The next largest number of cases under treatment were suffering, strangely enough, from that new disease with which many American women are now destined to spend the remainder of their days—post-operative pain and neuroses. It is a sad warning that eight cases applied for treatment for pains, aches and ill-health following removal of the ovaries or removal of tumors, in each case the condition being worse than before the operation. From two to five years had elapsed since the operation in each case, eliminating the mere climacteric symptoms that all have to go through. One of these cases was entirely relieved, three improved, and four remained unimproved.

Carcinoma.—One case of incipient, but undoubted, carcinoma of the breast was cured and has remained well. Two cases of carcinoma of the cervix were temporarily improved and one not improved. The value of zinc-amalgam cataphoresis is only now being tested in this affection, and it is my intention to make a full trial of the method in carcinomata that are strictly local.

The remaining cases of interest may be reported in tabular form :

	Cases.	Cured.	Improved.	Became Worse.
Subinvolution . .	7	7		
Menorrhalgia . . .	5	4	1	
Endometritis . . .	4	3	1	
Retroflexion . . .	2		2	
Retroversion . . .	2	1	1	
Pyosalpinx . . .	2	1		1
Hydrosalpinx . .	1	1		
Ectopic Gestation .	1	1		
Prolapse	1		1	
Urethral Caruncle	1	1		
Pruritus	1	1		

DISCUSSION.

Dr. T. J. Mays asked if Dr. Massey had applied electricity to a condition often met with in young girls who belong to phthisical families. This consists in a mucous discharge from the vagina and from the uterus and is probably, at least in part, due to a depraved constitution. It is often most rebellious to treatment and seems to be relieved sometimes by local medication and by efforts to build up the constitution, and if the constitution does not build up the results are not encouraging.

Dr. W. S. Stewart stated that he had not had any experience in the use of electricity himself in twenty years. He then began it for a while and his experience was so unsatisfactory that he gave it up altogether. He could not understand how the results described in pyosalpinx could be brought about—dissolution and removal of pus without any application of the knife or by the method presumed to be the only one of producing healthful resolution.

Dr. Massey added, in conclusion, that the existence of tuberculosis of the genital organs rather contraindicates the local use of electricity and indicates ablation by the knife.

In the treatment of pyosalpinx, the object should be to reduce the inflammation and open the tube in order that natural drainage may occur and that is what happened in the case that was cured. The patient presented great fulness in the region of the left broad ligament, and after one of the treatments a very copious yellow discharge took place, not in the nature of a steady drainage, but a sudden gush, following an application of thirty-five milliamperes to the interior of the uterus. The woman subsequently became pregnant and went to normal puerperium. Two years afterward she was doing well.

In the case of ectopic pregnancy, the diagnosis was made of extra-uterine gestation, with rupture into the broad ligament. There was no abdominal bleeding, but all the presumptive symptoms of an ectopic pregnancy were present. Cure followed arrest of the life of the fetus, the tumor shrinking and leaving finally only slight fulness over the ligament where the painful lump had been.

A CASE OF PROSTATIC ABSCESS.

JOHN LINDSAY, M.D.

[Read September 9, 1896.]

Abscess of the prostate gland is, according to Taylor, almost always a painful affection, and sometimes a dangerous and even deadly one. The cause of such an abscess is usually to be found in a preëxisting acute or chronic posterior urethritis, the passage of the inflammation from the urethral mucous membrane to the prostate being readily understood from their intimate anatomic connection.

During the formation of pus in the prostate the following chain of symptoms are generally present: Chills, fever, a feeling of fulness in the rectum, marked obstruction to micturition through the swelling of the prostate, as well as very painful defecation from pressure on the swollen gland—in other words, the symptoms will, as a rule, stand out so prominently that a diagnosis should be promptly and easily made. It is certainly desirable that an early and correct diagnosis be made in all cases, chiefly that the surgeon, if he so desires, may take an active part in saying where the contained pus shall be evacuated, for while the abscess, in more than fifty per cent. of cases, ruptures spontaneously into the urethra, yet, its contents may also find their way into the rectum, perineum, ischio-rectal fossa, inguinal region, or perhaps into the abdominal cavity, and thus lead to most serious consequences.

The following case is reported, not on account of its severity nor for any marked complications presenting themselves, but because the clinical picture was almost a negative one so far as being a guide to diagnosis.

A. H., aged twenty-nine; married; employed as waiter; consulted me at my office on August 14th, complaining of being chilly and having a feeling of soreness in his joints and muscles. This condition had existed for three or four days, together with considerable debility. Examination showed a temperature of 101° F., a coated tongue, but otherwise nothing of importance was elicited. The foregoing symptoms indicated influenza, if anything, and having prescribed on this basis and ordered a few days' rest, the patient was dismissed. On August 18th the man sent for me to see him at his house, and said that he felt no better, but rather that his symptoms were worse. At this time he mentioned, apparently as an afterthought, the fact that he believed he had piles. Inspection of the anus did not verify his statement, but examination per rectum revealed a general enlargement of the prostate, which was somewhat tender on pressure, and warm to the feel. The patient, whom I now questioned concerning past attacks of urethritis, denied ever having had gonorrhea in any form. In his recent history I could learn of no injury having been received, nor of any unusual exercise, as bicycling, being indulged in. He remarked that having recently removed, he had a long distance to walk to and from work. I now prescribed hot hip-baths, frequently repeated tonic doses of quinin, magnesium citrate and confinement to bed. The next

evening he had a severe rigor and passed a very uncomfortable night. On the following forenoon he had difficulty in urinating, with more or less violent pain, when the abscess suddenly burst into the urethra, when, of course, he was relieved, his febrile symptoms disappearing, and the act of urination being again freely established. I saw the patient soon after the abscess had ruptured, and found bloody pus flowing from the urethra, the discharge being much augmented by pressure on the gland per rectum. The quantity of pus evacuated was about one and one-half ounces.

The cause of this abscess was not obvious to me, and if the patient had not, somewhat casually, spoken of piles, my rectal examination would not have been made and consequently the diagnosis might only have been determined with the appearance of pus at the meatus.

I prescribed fifteen grains of boric acid three times a day, and ordered the patient to report at my office. He has thus far failed to do so, but I have learned that he is at work again and feeling well, so I hope that his abscess-cavity will granulate, contract and heal without further trouble.

As already stated, the clinical picture in this case is of interest from its lack of what might be called the classic symptoms of the disease, and also from the absence of any obvious cause. The case illustrates further the value of exploration of the rectum.

The fact that many cases of similar kind have terminated fatally, as a result of the abscess not rupturing into the urethra, renders any case, obscure as this was, of interest.

DISCUSSION.

Dr. G. B. Massey suggested that the abscess might have been in one of the vesicles, in view of the absence of pain.

Dr. Lindsay added that he had learned that the patient was at work, feeling well and free from backache or further trouble.

TREATMENT OF RETRODISPLACEMENTS OF THE UTERUS.

E. E. MONTGOMERY, M.D.

[Read September 23, 1896.]

The forms of retrodisplacement are version and flexion. I do not propose to enter upon the consideration of etiology and symptomatology, but it should be remembered that displacement *per se* does not necessarily induce symptoms and consequently is not an absolute indication for treatment. It is only when the malposition is accompanied by congestion, inflammation, or hypertrophy that abnormal symptoms are exhibited. It is often a question difficult of determination whether the displacement stands in the relation of cause or consequence to the complication. Many of the symptoms are undoubtedly due to interference with the circulation occasioned by the situation of the uterus at a lower level than normal.

The treatment consists in massage, mechanical measures and surgical procedures. The aim of treatment should be the restoration of the uterus to its normal situation and its maintenance there and the relief of symptoms. One of the earliest methods of treatment and one yet frequently pursued is the maintenance of the uterus in its normal position by the use of a *pessary*.

It should be remembered that the pessary does not correct the displacement and but serves to maintain the organ in a corrected position. A pessary consequently should not be introduced until the uterus has been carefully replaced. Use of the support without replacement but aggravates the disease and the discomfort. The organ is replaced by manipulation. This may be aided by the position of the patient, as the dorsal and the genupectoral. With the patient in the dorsal position and the limbs flexed, one or two fingers of one hand are introduced into the vagina, while the other hand rests upon the abdomen. The fundus is pushed up by the middle finger in the posterior vaginal fornix, while the index-finger hooks in front of the cervix and pushes it backward. This action upon the lower end of the uterine lever carries the fundus forward until it can be grasped by the external hand and brought against or beneath the symphysis pubis. The existence of adhesions will prevent the replacement, or lead to an immediate return of the abnormal position, so soon as the restraining force is removed. Not infrequently it will be difficult, or impossible, to dislodge the fundus from beneath the projecting promontory of the sacrum. The dislodgment may be facilitated by grasping the cervix with a tenaculum or vulsellum and drawing upon the uterus until the fundus can be pushed forward, when the cervix is carried back. The genupectoral position, with the lips of the vulva separated, permits ballooning of the vagina and carrying upward of the uterus, but not necessarily with correction of the position. Drawing downward and backward upon the cervix promotes a restoration of the position. This may be expedited by pressure upon the fundus through the posterior vagina. The position corrected, the pessary may be introduced at once.

A third method of replacing the uterus is by the use of the uterine sound or re-

positor. The danger of traumatism, infection and the production of sepsis is so great as to render this procedure inadvisable.

Care must be exercised in the use of the pessary to adapt the instrument to the patient and not the patient to the instrument. The proper length can be determined by measuring upon the examining fingers the distance of the posterior fornix from the posterior surface of the symphysis pubis; the proper width by separating the fingers. The Mundé or Thomas modification of the Hodge is the best form of pessary, in that each has a thick posterior bar. Those with a circular opening for the cervix are objectionable, as the weight will sometimes drive the organ into the opening until the pressure obstructs the canal and prevents drainage. The pessary does not maintain the normal position of the uterus by supporting the fundus with its posterior bar, but by a pulley-like action of the vaginal wall over the instrument.

Pushing up the posterior fornix draws upward the cervix and the other end of the uterine lever, the fundus, consequently falls forward. The pessary is only applicable to those cases in which the uterus is free and is readily replaced. Even in such cases the organ may be so tender that the instrument cannot be borne or the displacement has existed so long that the posterior vaginal fornix has become practically obliterated and affords no space for the lodgment of the pessary. In such cases a preparatory treatment with tampons to stretch the vaginal pouch may be necessary. Such tampons may be composed of absorbent cotton, gauze or wool, saturated with a glycerin compound such as the boroglycerid, or a solution of ichthyol. Prepared wool is the best agent for the purpose, as it possesses more resiliency than the other substances. It may be arranged in the form of a collar, being pressed well up behind, after replacement of the uterus, and the ends brought forward beneath the cervix. The medicated tampon, raising the organ to a higher level, improves its circulation and through the influence of the glycerin unloads the blood-vessels and promotes the absorption of acute inflammatory exudate. It has but little influence upon old or chronic inflammatory conditions. In the more acute cases it soon renders the use of the pessary practicable. In recent displacements, in the married or parous woman, the pessary may afford relief to all the abnormal phenomena of which the patient complains.

When the organ has been kept at a higher level until the circulation is improved, the organ decreased in size and the ligaments contracted, the patient may then be able to dispense with the use of the pessary. In the majority of cases, however, this must be continued for an indefinite period.

To the nervous, sensitive woman, the pessary is always a source of anxiety, and in all cases it should be frequently removed and cleansed. It is a foreign body and by its presence increases the vaginal discharge. Cleansing vaginal douches should be used, but the employment of douches containing the mineral astringents should be prohibited, as the salts are deposited upon the pessary, roughening its surface and rendering it a source of increased irritation. Not infrequently, the salts from the vaginal discharge encrust the instrument and the irritation causes granulations to spring up until the instrument is completely imbedded.

It is well to advise the removal of the pessary if it causes pain or increased discharge, and under any circumstances it should be removed for cleansing at least once in two months.

Massage of the uterus is productive of benefit in all cases of displacement. The manipulation and squeezing of the organ increase the activity of its circulation, promote the absorption of exudate and act as a stimulant of the muscular fibers in its ligaments. Massage finds its greatest efficacy in parametric inflammation and exudation. Fixation can be overcome and the organ rendered freely movable, but considerable time and patience are required for the accomplishment of these ends.

The practice of massage is contraindicated in cases presenting tubal collections of pus. As it is frequently difficult to determine the absence of

such collections, massage should be employed with great circumspection in all suspicious cases.

Surgical procedures. The operative treatment of retrodisplacement, has been a prolific field. It includes both extra-peritoneal and intra-peritoneal procedures. Sims advocated in cases of recent displacement that the uterus should be raised by a sound or repositor and the adherent gut pushed off by the external hand. Schultze advocated raising and fixing the uterus with the thumb in the vagina while two fingers are passed into the rectum above the fundus and the gut is dragged from the uterus by hooking forward and withdrawing the fingers; the organ is then drawn forward beneath the symphysis pubis, where it is maintained by a pessary. Both of these plans of treatment are objectionable in that they are blind procedures and render possible the tearing of a pus-tube or injury of an adherent ovary.

Shortening of the round ligaments, as advocated by Alexander and Adams, consists in fixing the uterus through extra-peritoneal incisions. Originally, the operation consisted in an incision over each external inguinal opening, picking up and drawing out and fixing the round ligament by sutures, the superfluous portions of the ligament being cut off. Various modifications have been introduced. Thus, only the external ring is exposed, the ligament is picked up at the internal ring and drawn out on a straight line; or it is exposed in the canal by slitting the muscle up to the internal ring. It is fastened by silk, silver wire, silkwormgut or catgut. The superfluous end is cut off, folded up beneath the subcutaneous fascia, fastened by a knot to the opposite ligament and buried in an incision connecting the two wounds, or the ends are pushed through the fascia and united by sutures. The operation is only indicated when the uterus is freely movable.

Some of the objections to its practice are: (1) That it is a blind procedure and may be performed when there are perimetric adhesions that will produce subsequent discomfort; (2) it requires two incisions and often considerable dissection; (3) the ligaments are often so attenuated that they are discovered with difficulty and are unserviceable as supports.

The principal advantages of the operation are that it can generally be accomplished without opening the peritoneum, and that, beyond some pain at the eighth month from traction upon the scars, it is not found to interfere with the processes of gestation and parturition.

The intra-peritoneal procedures are either abdominal or vaginal. The round ligaments are shortened within the abdominal cavity by suturing together a fold of the middle portion of either ligament (Wylie), by grasping the center of the ligament, stitching it to the side of the uterus (Dudley), or passing a suture beneath the middle of the ligament, through the anterior surface of the fundus and beneath the middle of the opposite ligament, which, when tied, brings the ligaments together in front of the uterus and swings the organ well forward (Mann).

The intra-abdominal operation most frequently practised is known as ventral fixation or suspensio uteri. The steps of the procedure are a short median incision, separation of adhesions, bringing forward of the fundus, examination and treatment of the appendages and fixation of the uterus. Two sutures are introduced through the fundus, fastening it to the parietes. Various methods of suturing are used, buried sutures of silkwormgut, catgut, silver wire and silk, or through-and-through sutures which are subsequently removed. The buried sutures include muscle and aponeurosis, without peritoneum, or the peritoneum alone. The continuous chromicised catgut suture for closing the peritoneum and fastening the uterus to it by the two lower turns, while with the same suture the muscle and aponeurosis are subsequently united, will prove very satisfactory. This procedure has the advantage over the Alexander operation in that it is (1) applicable to all conditions of the retrodisplaced organ, whether free or fixed; (2) it permits the careful examination and necessary treatment of the pelvic organs; (3) it

requires but one short and clean incision.

The only disadvantage is that it has been found, in a large proportion of cases, to disturb the normal course of subsequent pregnancy and parturition. It is a question, however, whether these disturbances are not a result of faulty procedure rather than the proper performance of the operation. A peritoneal band of union will be amply sufficient to maintain the organ and will subsequently elongate to such a degree that it will not interfere with the physiologic enlargement of the uterus.

Various efforts have been made to secure ventral fixation without opening the peritoneum. An incision has been made down to the peritoneum and the uterus pushed into the opening and sutured. Sutures have been introduced through the abdominal wall and fundus without incision, but all such procedures are blind and unsatisfactory, besides not being free from danger.

The instrument devised by Dr. Baum, of Kansas City, for passing a suture through the fundus and abdominal walls is only to be condemned; as is also the procedure of Shucking, which consists in springing a needle from a carrier through the fundus of an acutely anteflexed uterus and bringing it out through the anterior fornix of the vagina. A ligature carried back as the needle is withdrawn, when tied, holds the uterus in the anteflexed position. This plan presents the possibility of injuring the bladder or ureter, and does not fix the uterus in a satisfactory position.

Vaginal operations for correction of the displacement are done through either the anterior or posterior fornix. Dührssen's or Mackenrodt's operation known as vaginal fixation consists in a vertical section through the anterior wall of the vagina, pushing off the bladder until the peritoneum is reached, and the latter opened. Any adhesions of the uterus are separated by passing two fingers over its fundus and the organ is drawn forward with both fingers, tenaculum, or vulsellum. The anterior surface of the uterus is fastened to the vaginal wall. The operation maintains the uterus in many cases in a very satisfactory position, but the subsequent occurrence of pregnancy has been found to result in abortion or marked discomfort during the course of gestation, with ultimate complication of delivery. In some cases the uterus is brought forward with difficulty, and a number of cases are recorded in which the organ was so torn and injured as to render hysterectomy necessary. The operation has been modified by stitching the uterus to the bladder, thus obliterating the vesico-uterine culde-sac. Wertheim advocated utilizing the round ligaments through the vaginal incision. An incision is made similar to that in the Mackenrodt operation. The fingers are passed over the fundus, the appendages examined and a loop of first one and then the other round ligament is drawn down and sutured in the vaginal incision.

The operation of fastening the round ligaments to the anterior surface of the uterus, as suggested by Mann, may be performed through the anterior vaginal incision and the peritoneum and the vaginal surfaces brought by sutures in contact with the uterus.

For retroversion or retroflexion with adhesions, Pryor has advocated an incision through the posterior fornix, separation of adhesions by the fingers, bringing the uterus forward, and introducing a large packing of gauze behind the organ to prevent it becoming again displaced. Gottschalk commends shortening the utero-sacral ligaments. If these ligaments are shortened, the cervix is drawn upward and backward, and as a natural consequence the other end of the uterine lever is carried forward in anteversion. The method of procedure is as follows: A median vertical incision is made through the posterior vaginal fornix, beginning half an inch behind the os, and extending downward about two inches; Douglas' pouch is opened and the peritoneal vaginal incisions are united by temporary sutures, the ends of which are left long, and serve as retractors to keep the wound open. When adhesions exist, they are broken up, the condition of the appendages examined and treated, and the uterus replaced. The left utero-sacral ligament is fixed with the tip of the left forefinger as far

as possible from the cervix; an assistant keeps the wound open by strong traction upon the temporary sutures. Under guidance of the finger and control of the eye a suture is carried from above downward through the ligament and tied, one end of the suture is threaded into a curved needle which is made to enter the cervix a little below the level of the internal os, and, carried deeply, emerges near the margin of the vaginal wound. The same procedure is observed upon the opposite side, and the sutures are tied so that the knots lie in the vaginal fornix. A small strip of gauze is introduced into Douglas' pouch, and the wound is allowed to close by granulation. The patient is permitted to leave her bed on the seventh day.

In the treatment of retrodisplacements, as has been mentioned, the displacement is not generally of so much significance as the accompanying complication, and no procedure that does not take cognizance of the latter will prove a satisfactory method of treatment. The proper treatment for the varying conditions may be briefly outlined as follows:

1. In recent cases, with a freely movable uterus, the medicated tampon or pessary. In many cases, the former will be required as a preparation for the latter.

2. In recent cases with a plastic exudate and adhesions (when pus-tubes can be excluded), massage supplemented by the medicated tampon and restoration of the mobility of the uterus, followed by the use of the pessary.

3. In chronic cases, with a movable uterus, curettement, followed by suturing the round ligaments in front of the uterus through an anterior colporrhaphy.

4. When the displacement is complicated by disease of an ovary or tube, curettement, followed by abdominal incision, treatment of the affected appendage and fixation of the uterus to the abdominal wall.

5. When there exist adhesions without serious tubal or ovarian disease, curettement and shortening of the uterosacral ligaments after separation of the adhesions through the posterior vaginal incision.

DISCUSSION.

Dr. G. E. Shoemaker said that in considering the treatment of retroversion great care must be taken to determine whether or not the retroversion is the cause of the patient's symptoms. In many cases these are due to displacements of the ovary secondary to retroversion, to a relaxation of the broad ligaments, with a resulting venous congestion and a condition of edema and impaired circulation of both uterus and ovaries. If such steps are not taken as will replace the ovaries any treatment of the retroversion will be useless. The pessary is at best a temporary expedient and most men are trying to do without it as far as possible. It is never of use when there are adhesions and that is a point that is frequently lost sight of in attempts to use it. To this is due a large number of failures. The pessary is, however, useful in a small number of comparatively acute cases as a temporary expedient to hold up the uterus until involution can take place. When it comes to the permanent treatment of retroversion the question of the absence or the presence of fixation by adhesions must be determined, and it is a most important point. If adhesions are present such an operation as shortening the round ligaments is useless, and the pessary is useless. We are obliged then to do some cutting operation which opens the abdomen, frees these adhesions and then corrects the displacement. In Dr. Shoemaker's judgment the best operation for this is ventrofixation of the uterus. This designation is a misnomer. The operation is not one of fixation, but one of support; we form a band about three-quarters of an inch in length and of about the thickness of a lead-pencil or smaller, which allows free movement of the uterus, but prevents its falling backward and the consequent relaxation of the broad ligaments. It assists in the maintenance of the ovaries in their position. This fixation of the uterus has been said to interfere with pregnancy and there have been some cases reported of difficult labor. In Dr. Shoemaker's judgment this difficulty can be very largely obviated by putting the sutures in front of the line of the tubes instead of behind that line. This allows the fundus of the uterus to enlarge

upward into the abdomen without making extreme traction at the point of attachment. If one ties down the top of the fundus by sutures behind the line of the tubes only the posterior wall can enlarge upward during pregnancy. Dr. Shoemaker had two cases in which pregnancy occurred and in neither had there been serious trouble. He had done the operation many times. In one of the cases of pregnancy sterility had existed for seven years, disease of one ovary and tube with bifid uterus complicating the retroversion.

In Dr. Shoemaker's opinion vaginal operations are not usually advisable. In the first place, if the bladder is stripped off the uterus and this wound is allowed to granulate there is produced cicatricial tissue in a region which is exceedingly sensitive to traction, the base of the bladder. The operation is to a certain extent a blind one. The same objection applies to vaginal operations behind the uterus, especially if gauze is used and healing is obtained by granulation, which results in thickening. Dr. Shoemaker had had a great deal of trouble in cases with old cellulitis about the utero-sacral ligaments and he would hesitate to form scar-tissues in such an unfortunate place.

The Alexander operation is useful in a very few cases with freely movable uteri, but Dr. Shoemaker would further limit the operation to cases in which there is no descent. The Alexander operation has little effect in actually holding the uterus up in the pelvis.

Dr. J. M. Baldy maintained that retrodisplacements that cause suffering are almost always complicated, and complications of any kind whatever are contra-indications to the use of a pessary. A large number of retrodisplacements are unattended with symptoms and require no gynecologic treatment whatever. If a pessary be employed when a complication exists, the patient will, in a very large number of cases, not be cured and may even suffer an infinite amount of harm. The pessary, as well as minor operations, is responsible for a considerable number of cases of pelvic suppuration. Too often the complication is ignored while the retrodisplacement is recognized.

The use of the pessary may further be followed by the so-called "sore pelvis," in consequence of which many women's lives are made wretched.

The pessary has practically gone out of existence in the armamentarium of the nineteenth-century gynecologist.

Dr. Baldy characterized massage of the uterus as a mere refinement of masturbation. In his experience, it had done only harm. It is without use, except when a great deal of lymph is thrown out into the pelvis, when it may help stimulate absorption by gentle friction. To go beyond this would be to force the patient into more extreme inflammation and even into suppuration.

The disadvantages and actual harm resulting from the measures already mentioned leave practically only surgical treatment. When retrodisplacements are attended with symptoms, complications exist, and if these are ignored, little or no good can be done for the patient, while harm may result. The subject of surgical methods is yet an open one. There are but two operations, perhaps, that are worth considering—hysterorrhaphy and Alexander's operation. The rest are distinctly and absolutely bad. All of the vaginal operations are exceedingly dangerous. There is very little risk primarily, but the secondary effects are most disastrous. Cesarean section has been necessary to deliver patients thus operated on in whom the previous confinements followed a normal course; at times with death. Version and minor operations have been necessary sometimes in the same classes of cases. Of the two operations, hysterorrhaphy is decidedly the better procedure. It is, however, performed much oftener than it should be. The Alexander operation has a more limited field and for the same reason as the pessary—that is, the symptoms are always due to the complication and not to the displacement. The Alexander operation is followed by adhesions that give rise to bad symptoms. Even the advocates of this procedure admit that cases in which adhesions exist are not suitable for operation. It is often extremely difficult to determine whether or not adhesions exist in a given case. Hysterorrhaphy seems to be the best measure available at present, although attended with many dangers.

Dr. J. M. Fisher also agreed that there are few cases in which the pessary is at all useful.

In case of retrodisplacement of the uterus, the first question that should be asked is, What is the cause of the retrodisplacement? There has not been sufficient said of late years upon the prophylactic treatment of retrodisplacements. We know that the majority of displacements originate in the puerperal state. For example, laceration of the pelvic floor takes place and leads to subinvolution of the vagina and of the utero-sacral ligaments, and this continuing, at the time the patient rises from her bed the uterus drops downward and backward out of position. Although these organs may even undergo involution, a laceration of the pelvic floor generally involves tearing of the muscle which controls closure of the vagina. The levator ani muscle passes down along the sides of the vagina and around the posterior surface of the rectum, coalescing with the longitudinal muscular fibers and sphincter of the latter, and during defecation it serves the purpose of bringing counterpressure to bear in extrusion of material from the rectum. As it is the rule for women to be constipated, especially after having borne one or more children, considerable effort is necessarily brought to bear in defecation, so that

the uterus, under these circumstances, is forced down into the axis of the vagina. The latter being relaxed, because of want of support, the body of the uterus is tilted back, and the intra-abdominal pressure being directed against its anterior surface, adds insult to injury. If this is frequently repeated, retrodisplacement results as a permanent condition. Retrodisplacement occurring in this way cannot exist for a long time without inflammatory conditions of the uterus, as well as of the appendages arising because of the want of proper circulation in the pelvic structures. The pelvic veins are valveless and very tortuous, particularly the veins that pass through the pelvic floor. When a rupture takes place in the pelvic floor, these veins are drawn out and straightened, and the resistance that is normally offered to the force of gravity is removed. As a result, the vessels become dilated and varicose, and passive congestion of the organs takes place, and this, as is well known, is the first stage of chronic inflammation. Another factor in the puerperal state that is likely to give rise to retrodisplacement is a full bladder, or a full bladder in connection with a full rectum. Women are often allowed to go three or four days after confinement without a movement of the bowels, or a whole day or even a day and a half without having an evacuation of the bladder. When a woman is suffering from a distended bladder, as well as a full rectum, and in addition wears a tight binder, pressing the uterus into a posterior position, the organ being much enlarged and undergoing rapid involution, this condition, if maintained even for a few days, will determine a permanent posterior displacement of the uterus, because the uterus, with its supports, will accommodate itself to the new position. Much, therefore, can be done during the puerperal state to prevent the occurrence of retrodisplacements by sewing up lacerations promptly, by seeing that the bowels are emptied early, by attending to the evacuation of the bladder, and especially by not allowing the patient to wear a tight binder for eight or ten days. A binder should not be worn longer than twenty-four hours after confinement. Septic processes, as a matter of course, should be combated early. If the uterus is not in proper position, it should be replaced and be held in place by a properly fitting pessary. When a retrodisplacement exists, the uterus being movable and the woman becoming pregnant, or when a retrodisplacement occurs several days after confinement, during the process of involution, a pessary may be of use.

Dr. W. Easterly Ashton said that from a clinical standpoint, he divided all retrodisplacements, whether they were versions or flexions, into three varieties: First, those that are recent; second, those that are chronic; third, those that are attended with complications. He held that in recent cases and in thoroughly uncomplicated cases a pessary is the only form of treatment available. Thus after the puerperal period a woman with a perfectly normal pelvis may have a retrodisplacement, the utero-sacral ligaments being elongated. If a pessary be introduced, the weight is then taken off these ligaments and they have a chance to regain their normal tone and to contract and the uterine displacement is relieved. Just such cases are occasionally seen in a recent and uncomplicated state and they can be permanently cured by the use of a pessary. The number, however, is but a very small one. If an acute case is not relieved in the course of time by the use of a pessary, it then becomes an uncomplicated chronic case, and for this there is but one form of treatment. All cases, however, do not need treatment because all retrodisplacements do not give rise to symptoms. For those cases with symptoms as a result either of pressure or of interference with the circulation and for those characterized by a neurasthenic state, Dr. Ashton has but one treatment for and that is a surgical operation. Under these circumstances a pessary is not of the slightest use. Neither are gauze tampons, because after the case has existed for a certain length of time the utero-sacral ligaments have undergone fatty degeneration and will never perform normally their functions. Dr. Ashton never performs the Alexander operation because he does not believe that it will hold the uterus in place. This is especially true in those cases in which the round ligaments have been stretched for a long time and have undergone fatty degeneration. When the operation has been performed under these circumstances the uterus sinks again in a short time, on account of further relaxation of the ligaments and the case is in the end as bad as in the beginning.

From a large experience, Dr. Ashton concluded that the operation of ventrofixation is the most satisfactory in the whole range of gynecology. As performed to-day there is no other operation attended with the same brilliant results. Dr. Ashton attached little importance to the reports of bad results and complications following ventrofixation, maintaining that the operation as performed in the United States is rarely done properly. If in a ventrofixation the sutures are passed through the fundus of the uterus and are then carried through the peritoneum, muscle and fascia and are then tied, leaving them buried, the operation results in an absolute fixation of the uterus and it is incapable of yielding. If the sutures are passed directly through all the structures of the belly-wall the connecting band between the uterus and the parietal wall is so intimate and so strong that it virtually becomes a fixation. If an operation which is faulty in its technic produces a true fixation there is certain to be trouble in future parturitions. If the operation is done properly, and it is almost as delicate in its technic as

are some operations about the eye, the result is a ligament so delicate as not to interfere in the slightest degree with parturition and strong enough to act as a guy rope to keep the uterus in an anterior position.

In chronic cases it is necessary either to do something radical or to fall back upon the old plan of treatment by means of pessaries or plastic operations. None of the plastic operation devised upon the vagina or perineum is alone of the slightest use in correcting displacements.

In cases which are complicated, conditions apart from the retrodisplacement must be dealt with. If adhesions exist, the displacement is secondary; also, if an enlarged ovary or pustube is present, the displacement is secondary. These complicated cases are not to be viewed as examples of true retrodisplacement, nor to be approached clinically as such, but are simply to be considered from the standpoint of a gross pelvic lesion. In a case complicated by adhesions these are broken up and ventro-fixation is performed. When ovaries or tubes are diseased and require extirpation on both sides attempts at ventro-fixation of the uterus alone will prove useless. The best operation under these conditions is supravaginal hysterectomy.

Dr. G. Betton Massey contended that the pathology of inflammation as understood to-day assumes the necessity of microbic invasion. Two conditions are necessary for the occurrence of an inflammation : a deficiency of resistance on the part of the tissues and the presence of a germ; but nothing is said in pathology about an organ being out of place or about valveless veins. Mechanical views of treatment are the natural result of looking upon the mechanical deviation as the important point. The initial trouble in all cases of retrodisplacement is the inflammation of the uterus. The suffering is due not to a deviation but to an inflammation and its consequences, and the proper treatment is to get rid of the inflammation. The deviation from the normal position is merely a deviation of the position of an organ that is freely movable in the natural condition. The fault is therefore not the deviation, but the inability of the uterus to get back to its proper position by reason of the inflammation and added weight. The ligaments have also suffered from inflammation and have undergone fatty degeneration. The proper course of procedure should be the treatment of the inflammation, the reduction of added bulk, the absorption of adhesions so far as they can be gotten rid of, and the stimulation of the muscular supports. It is not necessary to tear apart the adherent surfaces. Who would think of tearing loose the adherent lung from the chest in an old case of pleurisy? Will not the surfaces so torn apart always unite? Pessaries ought not to be used for the further reason that even in acute cases they so dilate and distend the vagina, interfering with the natural action of the muscles as to render these incapable of performing their functions subsequently. Pessaries may be necessary in very old women who have already used them for years, and are amenable to no other measure. In Dr. Massey's opinion the best means of treatment is to stimulate the metabolic activities of the tissues through applications of electricity inside of the pelvis.

Dr. J. M. Baldy said that if the broad ligament is twisted on itself and carries the uterus and ovaries with it, the twist in the broad ligament will hold the fundus of the uterus posterior. Under such circumstances, relief from the acute suffering can be secured by replacing the uterus, when the broad ligament will untwist itself and the uterus will remain in place without any aid. In chronic cases, if fatty degeneration or stretching has taken place, when the uterus is placed forward again the broad ligaments will not hold it in position.

If the displacement be seen within the first week of its occurrence, and the uterus be replaced, a cure can be effected without the use of any support whatever, unless it be a soft wool tampon, keeping the patient quiet. A pessary, in some of these conditions, will tend to bring about displacement or relaxation of the ligaments by taking away from them their natural work, on the same principle that any tissue will undergo atrophy when not in use. No pessary introduced into the vagina will touch the fundus of the uterus when the uterus is in its proper position. Unless the pessary perforate the posterior cul-de-sac and enter the pelvis, it only gives support to the uterus by indirect pressure. The principal good a pessary will do, it effects by lifting the uterus up from below. In the acute cases, a wool tampon does this even better, with less chance of harm. The fact that the pessary is hard is an objection rather than a recommendation.

Dr. G. G. Davis expressed the opinion that the uterus, instead of being suspended by the utero-sacral and utero-vesical ligaments, practically floats in the pelvis, and that the ligaments probably prevent its being displaced posteriorly. At all events, when it is pushed posteriorly, fortunately the ligaments are put upon the stretch. The pathology of retrodisplacements is probably to be sought in some alteration of the pelvic floor rather than in the ligaments. If the uterus were supported from below, any alteration or disturbance of the pelvic floor would naturally allow it to be displaced to one side or other, or forward or backward. Such alterations could readily occur in labor followed by laceration of the perineum and other injuries.

Dr. W. S. Stewart found in the garments worn by women and the method of dressing the principal cause of all retrodisplacements.

Woman's waist has of late years been pushed down as low as possible and then constricted to the utmost degree. As a result, the abdominal contents undergo compression and displacement and the pelvic organs have their normal relations altered. When lacerations attend labor, the base of the pelvis gives way and further displacements result, not only in prolapse, but also in retroversions.

Dr. P. Fischelis said that many of the cases met with in general practice do not require operative interference. Displacements of the uterus arise also independently of the puerperal state.

Dr. E. E. Montgomery added, in closing, that the pessary is especially of advantage in cases of retrodisplacement of recent occurrence. Thus, in a woman shortly after confinement, with a uterus retrodisplaced and subinvoluted, the organ is heavy and remains subinvoluted as a result of the displacement, with more or less relaxation of the pelvic ligaments. If the organ is freely movable, it may be replaced and supported temporarily by a pessary, and through the higher level thus attained, the process of involution will be completed and the patient will in a short time be able to do without the artificial support. The uterus does not rest upon the posterior bar of the pessary. Anyone who introduces a pessary expecting it to lift up the uterus by pressing against its posterior surface converts a version into a flexion, and with the organ compressed between the posterior bar of the pessary and the anterior surface of the sacrum, the distress of the patient will be greatly increased. The failure to replace the uterus prior to the introduction of the pessary has been one of the causes of added trouble and distress rather than relief. The Alexander operation is necessarily of limited application, and should only be used when the uterus is freely movable, and can be readily replaced and maintained in its normal position without difficulty. If the displacement returns immediately after the removal of the restraining force, there exist bands of adhesion between the uterus and the rectum.

The objection to ventrofixation, that it forms a band between the uterus and the abdominal wall, which may subsequently interfere with the processes of gestation and parturition, is a legitimate one. If the union is made between a small portion of the uterus and the peritoneal wall, this band will elongate and the patient will be less likely to suffer in subsequent gestation and parturition.

Dr. Montgomery introduces one suture about the center of the fundus, in the transverse line, and a second suture just behind this, so that, practically, the uterus is brought into a state of anteflexion.

Massage properly employed is an aid in bringing about absorption of exudation and separation of adhesions, and relieves patients who would otherwise necessarily be subjected to operative procedure. It is a matter of regret that the surgeon cannot always imitate nature and reinforce the ligaments which suspend the uterus in its normal situation. Normally, the uterus is not in a fixed position; it moves about through a number of degrees—backward, forward, from side to side, upward and downward. If, however, the womb is the seat of a lesion that leads to its displacement, whether a bad laceration of the perineum or fatty degeneration of the muscular structures, whatever the cause, it is impossible by any other method of procedure to restore these tissues to their normal position and to sling the uterus once more in its normal place; consequently, it is necessary to do the best one can to place the organ in that situation in which it will give as little discomfort as possible to the individual, in which its circulation will be promoted and favored, and in which an inflammatory exudate and enlargement will be absorbed, and this is accomplished most effectually by one of the methods of fixation named.

While there is need for an operation that will not interfere with the subsequent processes of the individual, and that will aid in restoring her to as nearly a normal condition as possible, no method of procedure is as likely to afford greater relief than is procured through the operation of ventral fixation. The operation is, however, applicable only in a limited number of cases. In some cases, it is possible that the operation of fastening the round ligaments in front of the uterus will serve a useful purpose. It will not do so in every case, for the reason that the ligaments frequently undergo such changes from the long-continued duration of the displacement, that they do not afford sufficient resistance to serve the purpose.

THE PROPER POSITION OF CELIOHYSTEROPEXY IN GYNECOLOGY.*

FRANK W. TALLEY, M.D.

[Read October 14, 1896.]

There is, perhaps, no question in the field of gynecology upon which more diametrically opposite views have been expressed than that of celiohysteropexy. This operation has been extolled by some, condemned by others, and accepted with modifications by a few. Some definite place in gynecologic surgery it must occupy, and it is to define this position that I would ask the opinions of the members of the society.

An observation that, when the pedicle of an ovarian cyst had been fixed in the abdominal wound, it was common to find that a previous retroflexion would disappear, caused Kœberlé, in 1869, to remove a healthy ovary in a case of retroflexion and stitch the pedicle in the lower part of the abdominal wound. For the introduction of this idea into America we are indebted to Howard Kelly, who reported his first case at a meeting of the Philadelphia Obstetrical Society, November 4, 1886. Kelly sutured the left cornu of the uterus to the left of the median line of the abdomen, after having removed the left appendage, and recommended that in performing such operations, both cornua be sutured to the abdominal wall. This operation for the cure of obstinate retrodisplacement he considered to be established, although the indications for its performance would be, he claimed, rarely met. The operation has since that time suffered frequent modifications in its technic, and from a procedure rarely indicated had, until within a short time, become one of frequent performance.

Recently, much adverse criticism has been advanced. The principal arguments urged against the procedure have been:

1. That it is often followed by dragging pains which are so severe as to require subsequent celiotomy for the separation of the uterus from the abdominal parietes.

2. That it renders the woman liable to abort in the early months of pregnancy.

3. That subsequent labor is likely to be complicated, rendering forceps, turning, the induction of premature labor, etc., necessary resorts, and in some cases in which pregnancy had progressed to term, Cesarean section became necessary to effect delivery.

The dragging pains following celiohysteropexy, in some cases, may be explained by supposing that the uterus has been drawn, in the operation, too high up in the pelvis. If we consider, with Dr. Emmet, that the uterus, through its natural supports, occupies a certain plane in the pelvis, and that when prolapsed below this normal plane symptoms will arise from traction and tension upon the ligaments, we may realize that, also, if drawn above this zone the same traction applies, though in an opposite direction. In adapting the fixation, therefore, the distance above the pubic symphysis to which the fundus of the uterus will reach with ease must be determined, and should the operator make his attachment higher up the painful symptoms will occur, which are nearly as distressing as those before operation.

That celiohysteropexy predisposes to

abortion is an objection aimed at the method employed and not at the operation. If the uterus be fixed to the abdominal wall, either by unyielding buried sutures or by a broad, firm adhesion, it will not uniformly enlarge as the developing ovum demands. It is probable that the uterine contents may then be expelled. That this does not occur, however, in many cases is evident. Should it be possible to suspend the uterus in its normal position without firm fixation, providing for it a considerable latitude of mobility, such a danger would be minimized. Even should abortion result in a large number of subsequent pregnancies, the operation itself presupposes a previous retrodisplacement, in which state pregnancy probably would either not take place or be shortly followed by abortion. Thus, a large percentage of abortions after operation would be offset by the small percentage of births in women who otherwise would never have become mothers.

That labor is likely to be complicated and that Cesarean section should have been a necessary resort in a few cases after this operation, is again a fault of the method employed. Should it be possible to afford the uterus a light support which would permit a considerable latitude of movement, and which would either stretch or rupture during the enlargement of the pregnant womb, surely no serious result could be anticipated. When, however, the fundus of the uterus is broadly adherent to the abdominal parietes, either by fibrous connective tissue or by an unyielding buried suture, the enlargement of the uterine cavity takes place by the stretching of the posterior uterine wall, while the fundus being unable to rise in the abdomen, the hypertrophy of the anterior wall will rotate the cervix backward and upward as far as the utero-sacral ligaments and the posterior vaginal wall will permit, and then interpose itself as a thick muscular barrier between the fetus and the birth-canal. By carefully selecting a method of suspension by which firm fixation is avoided, these objections are overcome.

The operation that has afforded me the best results in child-bearing women consists in a small median incision under strict aseptic precautions, the separation of the uterus and appendages from their adhesions, the elevation of the fundus, the determination of the point above the pubic symphysis to which the uterus may be raised without undue tension upon its ligaments, and its suture with two stitches. For suture-material, boiled silk-worm-gut is preferred, passing the first strand through the entire thickness of the abdominal wall at the point selected, through the fundus of the uterus in a line midway between the two tubal attachments, embracing about one-half inch of the uterine tissue, and through the abdominal wall on the opposite side. The second suture is introduced similarly one-fourth inch above the first, through the abdominal wall and one-fourth inch behind the first one on the posterior aspect of the fundus. These stitches are tied tightly and the remaining wound in the abdomen is closed in the usual manner. The stitches in the abdominal wound are removed in eight days, while those holding the uterus are allowed to remain until the sixteenth day. The patient is preferably kept in bed for three weeks. It has not been my habit to use either tampon, pessary or abdominal support after the patient leaves her bed.

Some months after operation by this method, the uterus will be found to be restrained from prolapsing into its former position, yet capable of considerable freedom of movement, rendering it probable that an artificial suspensory ligament of some length must have been formed by the stretching of the adhesions of the two opposed surfaces of peritoneum. It has never been my lot to have reopened a patient upon whom I have thus operated.

Celiohysteropexy is indicated whenever an adherent retrodisplaced womb cannot be relieved by non-operative treatment, such as packing and massage, and is attended with such symptoms as disordered and painful menstruation, backache, bearing-down pains, obstinate headache, loss of appetite, difficulty or pain in defecation, and various general reflex nervous symptoms, which are not relieved by persistent local treatment.

Practically, this includes nearly all cases of adherent retroposed wombs. Unless the case be very acute when it comes into the hands of the physician, local treatment and massage will do little toward relieving the symptoms. Such women will require celiotomy to safely separate the womb from its adhesions, following which some means for its suspension must be devised to hold apart the denuded surfaces on the posterior face of the uterus and the posterior pelvic wall which corresponds to the adherent area. It is here that celiohysteropexy is indicated.

I do not believe that all non-adherent retrodisplaced wombs are productive of symptoms. Many wombs with relaxed broad ligaments and no peritonitic complications are found to be displaced posteriorly upon examination in the dorsal position, and would be found in anteposition if the patient were examined lying upon her abdomen. In such cases, frequently, the symptoms are dependent upon some other cause. A few of these cases may be relieved by a well-adapted pessary, which should be worn, if well tolerated, for months, with the hope that the ligamentary supports may regain their tonicity. Should they not do so after months of trial, celiohysteropexy is indicated, as I do not believe that any woman should be condemned to life-long use of a pessary. There is a class of neurasthenic women, however, in whom pelvic symptoms are bitterly complained of and in whom a retrodisplacement may be found, which is purely accidental to a primary neurotic condition. Such women are made worse by operative measures and are a tax upon the judgment of the physician.

In some cases the relief to the backache and the headache, which is frequently occipital or directly on the top of the head, is immediate. In other cases the improvement is gradual and may not be total for months. In none of my cases have I had troublesome bladder-symptoms resulting from pressure upon that organ.

For the relief of prolapse of the uterus in child-bearing women, celiohysteropexy affords a very important adjunct to the plastic operations for the narrowing of the vaginal caliber. After the uterus has been restored to the pelvic cavity, should the operator trust to narrowing the vagina alone, he will find that the uterus will in a relatively short time dilate its way and the prolapse return. This it does because the ligamentary supports have become overstretched and weakened, and the axis of the uterus coinciding with the axis of the vagina, intra-adominal pressure will urge it onward. Celiohysteropexy is then indicated as an adjunct to the plastic work upon the vagina in order that the axis of the uterus may be maintained at more or less of a right angle with that of the vagina, thus rendering descent impossible.

Usually in women with complete prolapse who have passed the menopause hysterectomy is the operation of choice. When this is for some reason undesirable, after the plastic operations upon the vagina, celiohysteropexy is preferably performed with buried sutures, providing for a more firm and reliable fixation.

In conclusion, I believe that celiohysteropexy, performed in the manner described, will produce an anteflexed uterus with a large area of mobility; that if the plane to which the uterus may be safely raised is estimated, it will not be followed by dragging pains; that during pregnancy there will be little tendency to abort, and that the suspensory ligament will either stretch or rupture during the enlargement of the uterus, so that no hindrance to delivery of the fetus will be encountered. I can recommend the operation for the relief of those symptoms dependent upon the retrodisplacement of the womb and consider it in the hands of clean operators a safe operation.

DISCUSSION.

Dr. Charles P. Noble said that, a short time ago, he collected from American operators over 800 cases of hysteropexy and studied the results which were secured. He had, himself, operated about seventy-five times and enough other cases had been operated on by Alexander's operation to make his experience cover about 100 cases. Dr. Noble has not observed pain after suspension of the uterus, and he believed it to be a comparatively infrequent symptom. Every surgeon has been unfortunate enough to have operated upon hysterical cases at times and upon neurasthenic cases in which it is sometimes difficult to determine the cause of symptoms present, but these are not to be included.

In the 800 cases studied there was absolutely no evidence that the operation favors abortion. The same was true of a larger series of cases collected from foreign medical journals. The whole series covered over 1500 cases altogether and in the whole number there was no evidence whatever that the operation favored abortion, so that in the absence of direct evidence this objection must be considered theoretic and without foundation.

The third objection, of dystocia, undoubtedly has a basis, and difficulty has arisen from too firm fixation between uterus and abdominal wall. The question is one of fixation and not of suture, because dystocia has been just as frequent when there was no buried suture as when buried sutures were used. In one of the American cases in which extreme difficulty was encountered in subsequent labor, there was suppuration after the operation of suspensio uteri; a large surface of adhesions was left and when pregnancy took place a tumor formed, obstructing the inlet of the pelvis. Dr. Noble had personal knowledge of two other cases, and in these, buried sutures were used.

If a technic is adopted that will attach the uterus to the abdominal wall lightly, dystocia will not occur, or but very rarely. As a matter of fact the percentage of difficult labor has been two per cent. in the series of American cases (that is *serious* difficulty not to be relieved by the ordinary resources of obstetrics)—a comparatively small percentage when the suffering that the operation is intended to relieve is considered.

Dr. Noble referred to one operator with whose results he is familiar, who made use of the through-and-through suture described. This operator also scarified the uterus. The result in his cases is that adhesions are so extensive that he is no longer as ardent an advocate of the operation as he was some time ago. The use of a through-and-through suture must favor the formation of more extensive union than if the buried suture is used, for the reason that the former draws the abdominal wall down tightly against the uterus and holds it there whatever number of days the stitches are left in, keeping a large surface of the uterus in contact with the abdominal wall and in that way insuring a large surface of attachment. On the other hand, if the peritoneum is lightly attached to the uterus the intra-abdominal pressure immediately after the operation tends to drag the uterus from the abdominal wall and in that way to form a light abdominal attachment.

Dr. Noble said that he is sufficiently conservative to find use for the pessary. A pessary is all that is called for in a small percentage of movable retrodisplacements. This is especially true in recent cases and puerperal cases when the uterus is large and heavy and involution has not taken place. Many cases recover under this method of treatment. All that is necessary is a certain amount of patience until good results are obtained. Very few patients with retrodisplacements of non-puerperal origin are cured, but many can wear a pessary comfortably. For movable displacements, if operation is necessary, Dr. Noble prefers Alexander's operation of shortening the round ligaments. This is better than the suspension operation because it gives a result that more nearly approximates the normal condition and is not any more dangerous than a suspension, if as dangerous. Dr. Noble has used both methods and judging from his own results he prefers the Alexander operation in this class of cases.

Dr. E. E. Montgomery said that the subject is an exceedingly interesting and important one. It is important from the influence that fixation is likely to have upon subsequent gestation and parturition. There is no question whatever, but that the method of treatment is capable of giving relief to women who would otherwise be victims of disease and distress, who have possibly spent years of suffering before the operation was done, and would be condemned to lives of invalidism were it not done. In view of the possibility, however, of trouble existing during subsequent parturition, it seems wise that the advisability of other methods of securing the same object should be considered. The cause of trouble is partly due to the pain and distress during the progress of gestation, and to thinning of the posterior wall of the uterus during the development of the organ, which renders pains or contractions subsequently irregular and necessarily increases the danger of rupture of the uterus. The method suggested of fixing the uterus would seem to result in either too firm or too slight union of the uterus and the abdomen. If the union is not firm, the removal of the sutures will be followed by separation and the uterus will

drop back, with redevelopment of the old condition.

In patients who are likely to become pregnant subsequently, it would seem to be a better plan of procedure to do the operation of shortening the round ligaments within the peritoneal cavity, as suggested by Mann. The uterus is thus swung in the pelvis, remaining perfectly movable, while at the same time it is brought forward. The organ is thus kept in place without the probability of trouble during gestation. In those who are not likely to become pregnant, the operation of ventrofixation may be done, as it can be performed more quickly. Dr. Montgomery referred to a patient who underwent the operation of fixation at the hands of a surgeon who uses a buried suture, and introduced the suture only between the peritoneum and fundus of the uterus. This patient has complained of a good deal of distress during gestation, from dragging upon the abdominal scar. She has reached the sixth month of gestation and is threatened with abortion. The latter is not due, however, to the fixation operation, but rather to having been run down by a bicycle. Upon examination of this patient, the finger could readily be passed through the cervix into the uterus and presented evidence of some flexion of the anterior wall of the organ, while the posterior wall was shortened and smooth, showing, if anything, that the dilatation of the uterus has been accomplished to some degree at the expense of its posterior rather than its anterior surface.

Dr. John C. DaCosta said that he had performed quite a number of suspensions of the uterus, and had had the privilege of examining some of the cases months after the operation. The patients in whom he had noticed pain were hysterical or very nervous women. This pain generally did not last more than a day or two. There were two cases in which it was accompanied by a little depression of the skin at the side where the sutures were put in, and which apparently pulled on the abdominal wall. In these cases difficulty in labor depends entirely upon the technic of the operation, and if through-and-through silkworm-gut sutures through the abdominal wall are used, and left for sixteen days, there is danger of dystocia. If the uterus is fixed firmly to the abdominal wall so that it cannot yield at all, irritation at least will result. Inflammation may be set up, as firm adhesion between the muscles of the uterus and muscles of the abdomen may take place as in a cut of any other part of the body. The trouble in these cases, and in the very two per cent. Dr. Noble speaks of, is due to the operation having been done with through-and-through silkworm-gut sutures and to firm adhesions.

The technic of the operation has a great deal to do with the successs or non-success. Dr. Kelly, in his last modification of ventrosuspension of the uterus, produces fixation by putting the sutures near the fundus of the uterus a little below the top of the posterior wall. Now, if, in introducing the sutures, not more than an eighth of an inch of peritoneum and fascia of the abdomen be included, and not more than from three-eighths to half an inch of uterus, not going more than a sixteenth of an inch into the uterus, and not using a heavy suture that will remain intact in the abdomen, but as light silk or catgut as will carry the uterus, putting the first stitch in the *top* of the uterus, very good results will be secured without the risk pertaining to the through-and-through silkworm-gut suture.

Dr. DaCosta referred to a case seen more than a year after operation, in which the retroflexion was so great that the fundus of the uterus was back against the cervix. Examination showed that the posterior wall at the internal os was thinned out to not more than once or at any rate twice the thickness of blotting-paper. A cystic ovary was removed, and suspension of the uterus was effected with very fine silk, including but little of the uterine tissue. At a later operation for omental adhesions by another surgeon the silk stitches were found to have disappeared, while the uterus was suspended by a long semi-serous cord, two inches in length, the viscus itself normal in position, perfectly movable, and its posterior wall, which had been so attenuated, had become normal in size. The uterus was perfectly able to carry itself and no longer required suspension. When such a condition exists there is nothing to prevent pregnancy or to interfere with parturition. A ligament of that kind, which will stretch that much in so short a time, cannot offer any obstacle to labor. In the case of another woman similarly operated upon, and five months later opened on account of tuberculous peritonitis, the same favorable condition of affairs existed. In Dr. DaCosta's experience these uteri do not drop back after the operation. The ligament is sufficient to hold them up and keep them from dropping back, and a very slight suspension does it. The uterus, being kept in proper position, is afforded the proper amount of nutrition, while the blood is able to get to it and it becomes normal in character. In some cases there is a little depression, which disappears at the end of three or four days, as if elongation had already taken place and semi-serous attachments were forming. If the uterus be pitched forward and stitches put through its back, pain is more likely to be present, as the uterus is not placed in a normal position, but an antedisplacement is produced.

Dr. J. C. Morris said that he had no experience with the operation of hysteropexy. He had relieved with pessaries such cases as came to hand; more especially retroflexions, almost without exception, had been relieved by intrauterine steam-pessaries properly supported.

For that reason he had never felt justified in subjecting patients to the other risks. Placing the uterus in position allows of normal nutrition taking place in the organ and relieves congestion, which is otherwise always present. If stimulation is desired in order to secure a more rapid result, a galvanic pessary, composed of zinc and copper, will frequently yield excellent results. Through these means many enlargements of the uterus have decreased, so that shorter and shorter uterine stems are required until the normal size is reached. In married women normal pregnancy has followed this treatment.

With such results, while not as brilliant as those obtained from operation and requiring a great deal of patience and perseverance, there seems no occasion to resort to surgical means.

ESSENTIAL PAROXYSMAL TACHYCARDIA—REPORT OF FOUR CASES.

ALOYSIUS O. J. KELLY, A.M., M.D.

[Read October 14, 1896.]

By essential paroxysmal tachycardia is understood a disorder characterized by the occurrence of paroxysmal attacks of abnormal frequency of cardiac action of unknown genesis. In cases presenting these manifestations there is demonstrable no lesion, either of the heart itself or of other organs, which latter, if present, might engender the symptom tachycardia. We thus distinguish between a disease-entity, so to speak—*essential paroxysmal tachycardia*—and that tachycardia which, although it may manifest itself in paroxysms, is but symptomatic of many and diverse diseased conditions of various organs—*reflex*, or *symptomatic tachycardia*. The subject may be illustrated by detailing the histories of four cases.

Case 1.—D. B., a native of Ireland and a quarryman by occupation, is a widower, aged sixty years. His father died of typhoid fever, his mother of some cause not ascertainable. He has had five brothers and one sister. Three brothers and one sister died of causes not ascertainable. Two brothers are living and are presumably well. He has had three children, two of whom are living and well; one died of some cause not ascertainable. With the exception of having had scarlet fever when young, the patient has always enjoyed good health. He never had rheumatism. His cardiac trouble is of ten or eleven years' duration. One day while working on a building he fell a distance of forty feet to the ground, alighting on his left side "near the heart." His ribs are said not to have been fractured, and he was confined to his bed for ten days after the fall. It was at about this time that he noticed something unusual with his heart. Since then he has had trouble with his heart of a similar nature, which usually manifests itself in attacks coming on at irregular intervals. These attacks, while they occur usually in the sequence of some exertion or emotional excitement, are frequently of spontaneous development. They are commonly ushered in by great palpitation of the heart and vertigo, the palpitation usually preceding the vertigo, though the latter is said to be at times the antecedent symptom, especially when a sudden lowering of the head precipitates an attack, as occasionally happens. The patient characterizes his greatest complaint as weakness, which is present during the attack and sometimes persists for a variable length of time afterward. This weakness is often excessive, but recovery is usually prompt. The patient says that during the attacks he has flushings of the head and neck, with subjective sensations of heat and sweating, but his statements are not to be entirely relied upon. The attack is usually accompanied by some dyspnea, but this is never marked. The man has no pain whatever in the region of his heart or elsewhere, no cough, no expectoration, no headache, no tinnitus aurium, no dimness of vision. The attacks may occur once a month or twice a week, and are apparently not of more frequent occurrence lately than they were some years ago. They last from an hour to

two days; usually about a half a day or longer. Some of the minor attacks so little incommode the man that he is able to go on with his work during their continuance. In some of the attacks he feels as though he would fall did he not support himself or receive assistance from others. He sleeps well, and is not nervous. His appetite is poor, and he has occasional epigastric distress and distention, with gaseous eructations after eating, but no nausea or vomiting or constipation. He has no symptoms referable to his genito-urinary organs. He denies having had any venereal disease. He uses alcohol and tobacco to excess.

On examination the man exhibits evidences of excessive indulgence in alcohol and exposure to all sorts of weather. He is tall and sturdily built; has a rugged countenance, with many minute dilated blood-vessels. He frequently exhibits some tremor following excessive potations, and his statements and answers to questions are as desultory and unreliable as is usual with ardent votaries of Bacchus. Physical examination of his lungs and abdominal viscera is negative. His urine is normal. His cardiac dulness reaches superiorly to the upper border of the left fourth rib at its junction with the sternum, and inferiorly from the left edge of the sternum to one inch within the left midclavicular line in the fifth interspace, at which point the apex-beat can be felt.* The auscultatory signs vary greatly with the time of observation. During the past ten years, his heart has sometimes been found by my father beating normally from seventy to eighty times per minute. Very frequently it is to be found beating between one hundred and two hundred times per minute, and this often without any apparent discomfort on the part of the patient. I have frequently counted it a hundred and ninety-two and again two hundred and sixteen beats to the minute, and have had the patient remark that his heart was then not bothering him at all. When the patient has what he himself recognizes as a severe attack, it is practically impossible to count the heart-beats. When beating with such excessive frequency, without much annoyance to the patient, the heart varies in action from minute to minute as many as twenty or thirty beats; and it will as suddenly drop from two hundred or thereabouts to a hundred and twenty as it will suddenly mount from a hundred and twenty to two hundred. It formerly used to drop suddenly to seventy or eighty and suddenly mount again to two hundred, and while this may still occur, it has not been observed lately, as the patient is seldom seen. There are no murmurs audible. The second aortic sound is somewhat accentuated. The peripheral arteries are a trifle hard. During the attacks of excessive palpitation it is almost impossible to count the pulse in the carotids or radials. As one would anticipate, the pulse is small and running, and while it can hardly be counted, one can appreciate that it is very frequent. Again, one is at times inclined to think that he discerns in the peripheral arteries about one-half the number of pulsations as there are distinct heart-beats detectable at the precordium; but this observation is not reliable. The left pupil is slightly larger than the right; both react well to light and in accommodation. The thyroid gland is not enlarged. Dermographism could not be demonstrated, nor factitious urticaria produced. There are no mottlings of the skin, and no hemorrhagic tendency.

Case 2.—F. H., a married white man, aged fifty years, is a native of Ireland, and a laborer by occupation. His parents and five brothers died of causes not ascertainable. One sister is living and well. He has himself had two children, both of whom are living and well. He always enjoyed good health, until fifteen or sixteen years ago, when he had rheumatism in his left hip, and he has been more or less troubled in the same way ever since. Seven years ago he had an attack of influenza, with marked pulmonary symptoms, which he thinks progressed to pneumonia, but this is doubtful. It was directly subsequent to this that his heart commenced to trouble

* This area of cardiac dulness has never been found to vary, either during or between the tachycardiac paroxysms.

him. He then began to suffer with attacks of excessive palpitation of the heart, accompanied at times by some precordial pain and dyspnea, a sensation of fulness in the head, headache, vertigo, dimness of vision, left-sided tinnitus aurium, flushing of the left side of his face and neck, with profuse perspiration in the same regions, anxiety, and frequent sensations as though he would fall. He is not known ever to have fallen, and never manifested any cyanosis, nor did he ever lose consciousness. The attacks were and frequently are precipitated by stooping at his work. They are of variable severity and duration, lasting from five minutes to a day. The minor ones may be repeated several times in one day. Since they commenced he has not passed an entire week without at least one attack of palpitation of the heart, which, however, is not always accompanied by precordial pain and the other symptoms already detailed. During the attack and for a variable length of time afterward, depending upon its severity, he is very weak. While the majority of the minor attacks, unless provoked by stooping or other manifest cause, develop suddenly, without any warning, he is usually able to presage the development of a severe attack by the occurrence of a premonitory epistaxis, which has frequently been of some profuseness. He has no cough and no expectoration. His appetite is good, although he has some epigastric distress and distention after eating. These, when excessive, sometimes induce an attack of cardiac palpitation. He has no nausea and no vomiting, and his bowels are regular. He has no genito-urinary symptoms, never had any venereal disease, and never used alcohol in any form whatever. He uses tobacco to a slight extent. About the beginning of May, 1896, he was suddenly seized with an attack which proved to be the most severe he had ever had. He was affected with great palpitation of the heart, accompanied by dyspnea and cough. He was exceedingly weak and much prostrated, and had a great fear of impending death, not associated with any actual pain about the heart, but rather with precordial distress. Repeated examinations of the lungs, abdominal organs, and urine gave always negative results. The apex-beat was faintly palpable in the fifth interspace, three-quarters of an inch within the left mid-clavicular line. The cardiac dulness reached superiorly to the top of the left fourth rib at its junction with the sternum, and inferiorly extended from the left edge of the sternum to the region of the apex-beat. The heart-beats ranged in number between two hundred and ten and two hundred and twenty per minute uninterruptedly for almost three weeks, and then gradually subsided to a hundred and fifty, and in the course of several days suddenly fell to seventy. During the following month the heart-beat varied exceedingly. It would suddenly mount from seventy per minute to a hundred and seventy, and as suddenly fall to seventy again. It would bound to two hundred or more and remain so for a few moments or an hour or more, and suddenly fall again to about seventy. No murmurs were audible. During the times of these excessive beatings it was almost impossible to count the pulse in the peripheral arteries; at all events no reliance could be placed upon the accuracy of the supposed results of such attempts. As the patient began to improve, gain strength, and become able to leave his bed, his heart-beat fell to sixty per minute, then to fifty-six, at about which rate it continued for a couple of days. Then suddenly one day while under observation it bounded to a hundred and seventy-six, to fall again shortly to seventy. Subsequently the man began again to improve and continued to do so, having occasionally attacks of slight degree and of not much more than momentary duration. While rather weak he was gaining strength. His pulse, however, continued somewhat arrhythmic, his heart-sounds were not clear, and his heart beat between sixty and seventy times per minute. On September 19th, following some excessive exertion, he was suddenly attacked with another paroxysm which was attended with vomiting. The heart-beat was two hundred and ten per minute. Digitalis and bromids were given,

and on the following day the man was very much improved, his pulse being forty-four per minute. There is no inequality of his pupils, both of which react well to light and in accommodation. There is no prominence of the eyeballs and no enlargement of the palpebral fissures. There is a marked tremor of the hands and fingers when they are extended. The thyroid gland is not enlarged. There is no hemorrhagic tendency, other than the epistaxis noted. Dermographism is readily demonstrated, but the markings do not persist long. Factitious urticaria could not be developed.

Case 3.—K. K., a white married woman, aged twenty-six years, is a native of Scotland, and a housewife by occupation. Her father had died of some cause not ascertainable, her mother of carcinoma of the pancreas.* Three brothers and seven sisters are living and well. She has had three children, all of whom are living and well. She has never been ill except when complaining of her heart. She does not recollect that it troubled her when she was attending school, but is certain that it did shortly after her withdrawal from school, when about fourteen years of age. Since then she has had attacks of palpitation of the heart, with or without dyspnea, slight vertigo, flushing of the head and neck, subjective and objective sensations of heat and sweating of the same regions. These attacks occurred irregularly about twice a month or thereabouts, and were usually sequential to some undue exertion or excitement. They would last about five or ten minutes, after which the patient would again be as well as ever. During her last pregnancy, which terminated in a normal labor May 16, 1896, she was subject to attacks of ordinary severity and duration. One week, and again two days prior to the birth of her child, she had attacks of much more than average severity and duration, both of which were attended with great weakness, persisting for some time after the cardiac palpitation had ceased. Twelve hours after her delivery she was suddenly, without apparent cause, seized with great palpitation of the heart, accompanied by the other symptoms already detailed and especially marked by great prostration. This attack continued for fifteen hours, and during its continuance the heart beat uninterruptedly between a hundred and eighty and two hundred and ten times per minute. It then suddenly fell to eighty, but the prostration continued for a day or more. There has since been no recurrence of the tachycardia. Physical examination of the lungs, heart, and abdominal viscera is negative, and the urine is normal. The thyroid gland is not enlarged, and there are no mottlings of the skin, no hemorrhagic tendency, no tremor. The patient has had no tinnitis aurium, and no dimness of vision. The pupils are equal, not dilated, and react well to light and in accommodation. Dermographism is easily demonstrated, but the markings fade after a few moments.

Case 4.—L. C., an unmarried woman, aged twenty-two years, is a native of the United States. Her father was killed in the coal-mines; her mother died at the age of fifty-seven years, of debility and gastric atony. Two sisters died in infancy of causes not ascertainable; one, aged seventeen years, of typhoid fever; one, aged twenty-two years, of pneumonia; and another, aged twenty-eight years, of rheumatism, with valvular disease of the heart. The patient had always enjoyed good health until three years ago, when her sister died of typhoid fever. Two weeks after this event the patient, in apparently good health, though greatly worried, was suddenly seized while in bed with great palpitation of the heart and marked dyspnea. These continued for three or four days, the heart beating one hundred and thirty-five times per minute. The patient then apparently recovered, and in two weeks was as well as usual. She then had another attack, accompanied by a chill, but she recovered in twenty-four hours. During the following six months she was usually very well during the day, but almost nightly she would have an attack of tachycardia, which, coming on about twelve or one o'clock, would continue frequently until daybreak. The attacks

* *University Medical Magazine*, November, 1895.

gradually decreased in severity and frequency until at the end of about six months the patient was able to do light housework. She remained free from severe spells for two years, when she had another, which, however, did not continue long. The patient states that her first severe attack occurred during the menstrual period, and even now the recurrence of this period is very prone to be attended with some palpitation of the heart. She began to menstruate when fifteen years of age (two years prior to the first paroxysm of tachycardia). Menstruation is and has been usually regular and painful. She always sleeps with her head high for the purpose of hindering the development of attacks of tachycardia. She is careful to guard against "cold," which brings on a mild attack. She is, in consequence, worse during winter. She never had rheumatism, but is occasionally "dyspeptic." Physical examination of heart and lungs reveals no deviation from the normal. Her urine is normal. She is nervous, annoyed by trifles, and asserts that her greatest enemy is "worry." She has no enlargement of the thyroid gland, and no exophthalmos. She says she has no flushings unless the attack be very severe, when she is also greatly prostrated. Any exhausting work brings on an attack.

The cases herein reported present, upon critical examination, certain very interesting features. While symptomatically they possess many points in common, each case manifests certain individual peculiarities. Two of the patients were males and two females. Their ages at present range from twenty-two to sixty years (1, 60; 2, 50; 3, 26; 4, 22). At the time of onset of the affection their ages varied between fourteen and fifty years (1, 50; 2, 43; 3, 14; 4, 19;) and the affection has lasted in the individual cases from three to twelve years (1, 10; 2, 7; 3, 12; 4, 3).

The essential feature of each case is the paroxysmally recurring attacks of extraordinary rapidity of the heart's action, without adequate obvious cause, predisposing or exciting. Certain of the paroxysms, however, are preceded by various manifestations which, while in many instances obvious, can hardly be looked upon as adequate to call forth the attack in the particular individual. In cases 1 and 2 there is a more or less intimate relationship between the spells of vertigo and the attacks of tachycardia. The two manifestations may occur simultaneously or independently of each other, and either may precede the other in time of development. The vertiginous spells are sometimes engendered in both patients by physical activity, such as lowering of the head. Again they develop without manifest excitation, occasionally preceding in time of development the attack of tachycardia. They might possibly be looked upon as causal of the latter, but they may not inappropriately be considered part of the symptom-complex of the tachycardiac paroxysm. It is quite probable that any sudden effort or change from ordinary conditions suffices to occasion the vertigo, and it is after this manner that the excessively rapid heart-action operates.

In cases 1 and 3 it is impossible to assign any cause for the development of the affection. In case 1 the disorder is said to have manifested itself subsequent to a fall, but it is extremely improbable that any real association exists between the disease and the accident. The disorder in case 2 is said to have come on directly after an attack of influenza. There are no sound reasons for denying that the influenza had a positive influence in the development of the disorder in this case. In this respect the case resembles those lately referred to by Sansom,[1] who, in a study of one hundred cardiac cases due to influenza, found thirty-seven cases of tachycardia, twenty-five of arrhythmia, twenty-three of pain, five of bradycardia, and ten of organic disease of the heart.

In neither case can alcohol or tobacco or other toxic agent be held answerable for the disease. The patient of case 1 overindulges habitually in alcohol, and uses tobacco to great excess. The patient of case 2 has been a total abstainer from alcohol for at least the past twenty-five years, and smokes but little. The patient of case 1 has made repeated experiments to determine whether or not alcohol had any influence on the severity or fre-

quency of his attacks of tachycardia. He found that he had on an average as many paroxysms when not drinking as when drinking; nor could he establish any causal relationship between indulgence in alcohol and the individual attacks. A severe attack might or might not follow excessive potations, and if an attack developed during a temporary interval of abstinence, repeated experiences showed that neither its duration nor its severity was with certainty influenced one way or the other by immediate indulgence.

The precise time of the development of the disorder in case 3 could not be ascertained, nor could any cause be discovered. The severe attack came on twelve hours after parturition, to which we can hardly assign any causative influence. The two occurrences were separated by a considerable interval of time (twelve hours), and the woman had previously been twice confined without the development of any such cardiac symptoms, although they were present at other times. Cases similar to this have been reported by Stocker,[2] Gerhardt,[3] and others. In none of these three cases is it possible to discover any reflex cause capable of inducing the paroxysms.

Case 4 we are disposed to view as one on the border-line between essential paroxysmal tachycardia and reflex tachycardia. The first attack came on during menstruation, and each menstrual period is still likely to be attended with some increased frequency of heart-beat. At some menstrual periods severe paroxysms of tachycardia occur, and many spells develop at other times from unknown causes. While in many respects this case appears to be one of tachycardia reflex from the female genitalia, it is more proper to view it as one of those instances of tachycardia, many analogous examples of which we have in other disorders, particularly of the nervous system, in which an affection takes its origin in a definite cause and persists whether or not the inducing cause remains operative. Finally, we must refer to the frequency with which, in all the cases, attacks are precipitated by excessive exertion.

In contradistinction from what might *a priori* be anticipated, these spells of abnormal frequency of the heart-beat are unaccompanied by marked dyspnea or cardiac pain or distress. On the other hand, when the attacks are severe there are manifest signs of general bodily disturbance, as evidenced by excessive weakness, which is entirely independent of any possible prostration engendered physically from fear of impending bodily calamity. This is seldom or never present.

Attention has been directed to the suddenness of the onset of the individual paroxysms, and to the almost equal suddenness with which many of them terminate. Again, other attacks decline much more gradually. During some of the spells the heart-beat is subject to marked variations in frequency, and these develop and cease as rapidly as do the attacks themselves. The paroxysms are of varying duration—from a moment to three weeks or more.

Particular attention is to be directed to the fact that in none of the cases was it possible to discover any cardiac lesion whatever. In cases 1 and 2, in which the area of cardiac dulness was determined during paroxysms, this was found normal, as it was also between the attacks. It was also normal between the paroxysms in the other cases, in which no notes were made of any exact observations upon this point during a seizure.

Finally, we must refer to the very manifest evidences of general vaso-motor disturbance in all the cases. These were particularly marked in cases 1 and 2. They consisted for the most part in subjective and objective sensations of heat, confined especially to the head, face, and neck and accompanied by profuse perspiration, limited to the left side in case 2 and said to be general in cases 1 and 3. These are very evident symptoms of vaso-motor paralysis. As further indicating the vaso-motor instability, we refer to the ease with which dermographism was demonstrable in cases 2 and 3; to the marked susceptibility to the influence of cold in case 4; and to the occurrence of epistaxis premonitory to the tachycardiac paroxysms in case 2. None of the other cases exhibited any hemor-

rhagic tendency. Even after centrifugation, no erythrocytes were discernible in the urine of any of the patients. These vaso-motor disturbances are asserted to have been absent in some of the reported cases, but in the majority of instances but little attention has apparently been directed to them. Rather extended reference to the subject has been made in a communication of Zuncker[4].

Interesting as is the symptom-complex of this affection clinically, by far the most interesting feature in connection with it is that concerning its pathogenesis. An important advance was made in the study of the subject when Fraentzel[5] and Bouveret[6] drew attention to the necessity of distinguishing between tachycardia as a symptom and tachycardia as a disease. As, however, the term tachycardia is still indiscriminately employed to designate diverse morbid conditions, it is primarily essential to define that to which we apply it. Tachycardia (more properly polycardia), meaning rapid action of the heart, in practical application should be employed to designate such a condition. It is at once evident that all instances of rapid action of the heart are not of the same nature nor due to the same cause. The term tachycardia therefore requires some qualification to indicate its nature in particular instances. Rapid action of the heart may be a permanent or a transitory affection. If the latter, it may, or may not, occur in paroxysms, and if the former, the permanent tachycardia may be augmented by the occurrence of acute exacerbations of greater frequency of heart-beat than is usually present. Of permanent tachycardia, numerous instances have been recorded. The cases that after death have been subjected to careful pathologic study have usually disclosed some lesion of the cardiac centers in the medulla, or of the vagus trunks or neighboring structures.

Tachycardia, which is but transitory, may occur in paroxysms or it may not. Under either condition it may be symptomatic of various diseased conditions of the different organs. When, however, it occurs in paroxysms of unknown genesis it is distinguished as essential paroxysmal tachycardia.

Symptomatic or reflex tachycardia may be due to a variety of causes, viz.: 1, anemia; 2, fever; 3, acute infectious diseases (diphtheria, scarlet fever, typhoid fever, influenza, etc.); 4, chronic infectious diseases (syphilis, tuberculosis, malaria, etc.); 5, organic diseases of the heart (of the endocardium, pericardium, or myocardium); 6, various emotional disturbances (shock, fright, etc.); 7, intoxications (tea, coffee, tobacco, alcohol, etc.); 8, nephritis, especially at a late stage of the cirrhotic form; 9, rheumatoid arthritis; 10, exophthalmic goiter; 11, chorea; 12, epilepsy; 13, hysteria; 14, neurasthenia; 15, masturbation and excessive sexual indulgence; 16, organic disease of the nervous system, brain, spinal cord, or nerves; 17, various reflex influences emanating from the brain, heart, stomach, liver, intestines, lungs, uterus, ovaries, gall-bladder, kidneys, prostate gland, urinary bladder, anus, brachial plexus, aspiration of ascitic effusion; 18, convalescence from any protracted disease. In view of the multiplicity of affections just cited, and which could be added to, the unwisdom of attempting a discussion of the pathogenesis of tachycardia is evident. The pathogenesis of such tachycardias is practically the pathogenesis of disease in general. When we attempt to classify such tachycardias we meet with new difficulties. Is the tachycardia of scarlet fever due to the fever, to the general infection, to the circulation in the blood of some toxin exerting a deleterious influence on the medulla, the pneumogastric or sympathetic nerves, the intracardiac ganglia, or the heart-muscle? Is the tachycardia of typhoid fever due to the fever, to the general infection, to the anemia, to the heart-weakness, or to what? Many of the tachycardias enumerated are justly regarded as natural concomitants of the affections they attend, and others are so manifestly reflex that they cease with the non-operation of the inducing cause —*cessante causa, cessat effectus.* These tachycardias are common, as attested by the daily experience of most practitioners. Within the past three months I

have, in private practice, observed two cases in which the heart-beat was respectively 135 and 146 per minute, in both instances during alcoholic debauches. It is interesting in this connection to note that Déjérine[7] has demonstrated in some of these alcoholic tachycardias, degeneration of some of the fibers of the vagus. At the Medical Dispensary of the University Hospital, I saw lately a patient with paroxysmal attacks in which the heart-beat ran up to 135 per minute as a reflex consequent upon catarrhal gastritis. I have lately reported another case in the *Philadelphia Polyclinic*.[8] The patient had aortic insufficiency and became the subject of apoplectiform bulbar paralysis, subsequent to which his attacks of rapid heart-beat were much more frequent than previously.

Accompanying all of the conditions already enumerated, tachycardia can be referred to some diseased condition of the body in general or of one or more of its organs. It is therefore symptomatic or reflex, and ceases as soon as the influences upon which it depends have been removed. Quite different and distinct are those cases designated essential paroxysmal tachycardia, which are so called because neither clinically nor pathologically can any cause be discovered to account for the development of the affection.

In studying deviations of the heart-beat from the normal it is well to commence with a consideration of the fundamental principles of the cardiac mechanism. The strength, rhythm, and frequency of the heart-beats are dependent upon the accelerating influence of the sympathetic nerves, the inhibiting influence of the vagus, and the regulating influence of the intracardiac ganglia. To these influences must be added that dependent upon the state of the nutrition of the heart, and that which the vaso-motor nerves exert by their influence upon the contractility and distensibility of the blood-vessels. In addition, experiments have of late tended to show that the heart-muscle itself possesses the inherent property of automatically contracting independently of any nervous influence, and that the rhythm of the normal cardiac action is dependent upon the functional activity of the intracardiac ganglia (Landois.[9])

It is not probable that any disturbance of the intracardiac ganglia could induce the paroxysms; nor is it probable that they are even secondarily involved to any marked degree. As remarked, they preside essentially over the rhythm of the cardiac contractions, and in the vast majority of the seizures in essential paroxysmal tachycardia the heart-beat and the pulse are regular (Huber.)[10]

The heart-muscle itself cannot be much altered. A few cases have come to necropsy, notably those reported by Riegel,[11] Finney,[12] Bristowe,[13] Fraentzel,[5] Brieger,[14] Sollier,[15] West,[16] Hochhaus,[17] and others. In the majority of the cases, post-mortem examination of the heart either yielded negative results or revealed but slight deviations from the normal. In none was there discovered any characteristic lesion or any lesion that with any reason could be held accountable for the production of the disorder. In several instances there existed some degree of interstitial myocarditis, which fact led West[16] to formulate the opinion that paroxysmal tachycardia is "not due to functional disturbance alone, but to an organic lesion, and that that lesion is in all probability in the muscular substance" of the heart. This view has found but few supporters, among them Brannan,[18] and is manifestly untenable. The interstitial myocarditis is much more likely an accidental concomitant. If it bore a causal relationship to the tachycardia we should find the latter of very frequent occurrence. It is also well known that even extensive degrees of interstitial myocarditis frequently exist without occasioning any symptoms whatever. Irregularity and frequency of the heart-beat and the pulse are usually the more prominent symptoms when any are present, and the former is much more likely to be encountered and is always the more marked. Further, it is not at all improbable that the persistence and frequent repetition of the tachycardiac paroxysm may be the cause and not the result of the interstitial myocarditis.

There remain then to be considered

paresis of the vagus and stimulation of the sympathetic. It has been demonstrated that paralysis of the sympathetic has no influence whatever over the cardiac mechanism (Nicati),[19] whereas stimulation of the cervical sympathetic gives rise to increased frequency of the heart-beat. In the cases herein reported, excepting the increased frequency of the heart-beat (referable to other causes), there is no evidence whatever of sympathetic irritation. All of the cases manifest signs of vaso-motor disturbance, and all of these signs are paretic in nature. We therefore conclude of necessity and by excluding other possibilities, that the seizures of essential paroxysmal tachycardia are due to some transitory disturbance of the functional activity of the vagus—most probably the center in the medulla. The disorder is a most extraordinary one. Concerning its manner of development we can but theorize. There are doubtless many instances in which the heart-beat does not become so excessively frequent, remaining about 120 or 130 per minute, and which may be due to sympathetic irritation. Such, for instance, is a plausible explanation of the phenomena of case 4. There occur in all likelihood, other cases in which the symptoms are due to both paresis of the vagus and stimulation of the sympathetic, as originally suggested by Gerhardt, who is of the opinion that the majority of all cases of tachycardia are due to paresis of the vagus, those with excessive pulse-frequency to paresis of the vagus and stimulation of the sympathetic, and minor forms with a less frequent pulse-rate to stimulation of the sympathetic alone.

The unreliability of using as a criterion the rate of the heart-beat in endeavoring to distinguish between the various causes or natures of the tachycardiac paroxysm led Nothnagel[20] to formulate the following two rules: (1) If during the tachycardiac paroxysm, the pulse-rate is very high, the rhythm maintained and the heart-impulse weak; if there are present no other symptoms, except such as may be regarded as secondary to incomplete emptying of the heart; and if finally there be present a paresis of other nerve-tracts running in the vagus, in the particular case we may assume a paresis of the vagus. (2) If during the tachycardiac paroxysm, the heart-impulse is strong; if the peripheral arteries are well filled and firm (not absolutely essential); and if there be present other manifestations of vaso-motor irritation, the supposition of an excitation of the accelerator nerves is justified. Although these rules are in accord with the facts regarding the apex-beat in cases 1 and 2, that they are hardly generally applicable is evident from the fact that in many of the reported cases supposed to be due to paresis of the vagus, the cardiac impulse was strong. There exists, in many cases, a remarkable difference between the apparent strength of the apex-beat, and the small, weak, scarcely perceptible pulse.

Again, Fraentzel has proposed the dictum that a paroxysm relieved by an injection of morphin may be considered to be due to sympathetic irritation, while one relieved by digitalis depends upon paresis of the vagus. But neither of these rules can be relied upon. Single paroxysms in the same case may at one time or another be relieved by either of these medicaments, and again at times neither will prove of any service. The affection manifests itself in paroxysms, many of which develop and cease absolutely, unaffected by any therapeutic measure.

Martius[21], in a recent monograph on the subject, holds that the attacks of essential paroxysmal tachycardia are due neither to stimulation of the sympathetic, nor to paresis of the vagus, nor to both, but that the essential nature of the affection consists in an enormous, acutely occurring dilatation of the heart. According to him there occurs first a lessening in the tone of the cardiac muscle, resulting in great dilatation. At the same time the physiologic property of the heart to produce maximal contractions is lost. There ensue therefore partial contraction and incomplete emptying of the ventricle at each systole. Of necessity there must result slowing of the circulation and dyspnea, unless the weakness of the contractions

is compensated for by their increased frequency. The tachycardia is therefore of the nature of a life-preserving compensation. Hochhaus concurs in the same opinion.

The majority of authors, however, with Bouveret, Huppert,[22] Winternitz,[23] and Sansom, view the dilatation of the heart as a purely secondary and not necessary manifestation of the paroxysms. Sansom, in reporting forty-six cases, asserts that, in the majority, the heart-dulness was normal. "In three cases only were there signs of dilatation, and the probabilities seemed strong that such dilatation was the consequence of the perturbation, and due to no other cause." That dilatation of the heart is not a necessary concomitant of the paroxysm is further very evident from a recent communication of Loeser.[24] In confirmation of this view I may mention cases 1 and 2 of my own, which were studied with particular reference to this sign, and in which dilatation of the heart was never observed, although looked for during paroxysms. Dilatation of the heart may or may not occur during a paroxysm. In a particular individual it may be present during one attack, and absent during another. There are many reported cases in which the affection persisted for years, and in which no dilatation ensued, although it did develop in other cases. The facts therefore do not warrant the deductions of Martius, and dilatation of the heart cannot be considered a pathognomonic sign of the paroxysms. Wood[25] likens the tachycardiac paroxysm to an epileptic seizure, ascribing its development to a discharge of nerve-force and not to any temporary paresis of the vagus. This view, however, has not found general adoption.

We come, therefore, to regard the vast majority of the paroxysms as due to a transitory paresis of the vagus, a view warmly supported by Rosenbach,[26] and many others; and we believe, further, that the locus of the disturbance is in the medulla. We have arrived at this conclusion by exclusion, but there are, in addition, many positive facts that corroborate this view. In many carefully reported and trustworthy observations, evidence has been adduced to show that the central tone of the vagus, the removal or loss of which occasions the paroxysms, may be compensated for in many ways, and the attack brought to an immediate end. It has been repeatedly demonstrated that, in some cases at least, it is possible, by means of mechanical irritation of the vagus in the neck, as by pressure, to bring the paroxysm to a sudden termination, or to at least reduce very materially the excessive frequency of the heart-beat. The same result has been achieved by applying an electric current to the vagus in the neck, or even by the simple procedure of swallowing a glass of cold water or a cupful of hot coffee. All of these facts serve to indicate that the peripheral terminations of the vagus are intact, or at least capable of responding to stimuli. The disorder is, therefore, due to a disturbance of the functional activity of the cardiac centers in the medulla, whereby the normal influence exerted by physiologically active pneumogastric nerves upon the heart is no longer operative. We look upon the associated symptoms in the cases herein reported—those indicative of vaso-motor paresis, as additional support of this vagus theory. The nuclei in the medulla presiding over the vasomotor activities are in close juxtaposition to those governing the cardiac mechanism, and as they are intimately associated anatomically, so also are they closely related functionally. It is, therefore, but fair to assume that any influence which seriously disturbs one might exert considerable influence also over the other.

The affection is a neurosis of the vagus. But what do we understand by such a neurosis? How does it operate? What is the *modus operandi* of the development of such sudden changes from a normal pulse-rate to 200 or more per minute, and of equally sudden changes from 200 to 70? It is in this connection that I venture to propose as explanatory of the phenomena of this disorder an adaptation of a theory lately advanced by Dercum,[27] Robl-Rückhard, Lépine, and Duval. It is that which contends that the neuron is not an absolutely fixed morphologic element. This theory

receives considerable support from certain demonstrated facts. Wiedersheim (quoted by Dercum) observed the nerve-cells in the esophageal ganglia of living animals of very low order to be capable of movement, which he describes as slow and flowing. A single observation of this nature is of the utmost importance, and serves to counteract theorizing to the contrary. In the words of Dercum, "I do contend that it is in the highest degree probable that such facts as we have, scanty though they be, are in favor of the view that a certain amount of movement does take place in the terminal portions of their processes [of the neuron], their dendrites and their neuraxons, although this movement is probably small in extent."

We now know, thanks to the investigations of Golgi, Ramõn y Cajal, Lenhossek, Van Gehuchten and others, that the nervous system is made up of a number of distinct neurologic entities — the neurons. These are as distinct and individual as other cells throughout the body. Their processes—dendrites, neuraxons and collaterals—do not fuse and anastomose, as formerly believed, but they are sharply defined and limited. They bear no relation to similar processes of other cells save that of proximity, or possibly contact, by means of their arborescent filamentous terminations. It is at least these filamentous terminations that we believe capable of motion. They possess the properties of extension and retraction. Properly extended these filamentous terminations of one neuron stand in normal relation to the filamentous terminations of other neurons with which they are in physiologic accord. The functions of the body are thus maintained under the influence of the nervous system and are properly executed. When, however, these terminals of neurons for any reason retract, other neurons, which under normal circumstances receive from the first-mentioned class impulses necessary for the proper execution of their functions, lack these essentials, and are no longer capable of normal physiologic action. Transmission of impulses cannot take place unless the terminals are in a certain degree of proximity. More widely separated, interchange of action ceases. This is the theory adduced by Dercum to explain many of the phenomena of hysteria, hypnotism, sleep, etc.; and it is this same theory that I believe most fitly explains the phenomena of essential paroxysmal tachycardia. The loss of tone or control of the vagus is thus accounted for, and particularly the suddenness with which it is lost, and the suddenness with which it is regained. This excessive motility of the terminal filaments may be induced by many causes, as for instance sudden excessive demands beyond the functional capacity of the cell, or sudden disturbance of normally existing conditions, such as may be engendered by excessive exertion or sudden stooping, as in our patients; or again defective nutrition or fatigue, which lessens the functional activity of the neuron, and thus perhaps as a life-preserving measure they retract their terminals. The instant they reach that degree of separation through which impulses cannot be transmitted from one to the other their influence suddenly ceases. Subsequently, when nutrition has restored the cell to its normal status, protrusion of the processes takes place, functional activity and the normal relationship of physiologically allied neurons is once more established, normal conditions exist, and the tachycardiac paroxysm suddenly ceases. Temporary reductions in the rate of heart-beat during paroxysms are abortive attempts of physiologically incapable cells to again assume their functions. Nutrition has not yet restored them to that condition necessary for physiologic activity, and they fail in their endeavors until fully replenished. There are admittedly many loopholes in this theory, but it appears to me attractive, and certainly as plausible as any, and much more so than many, that have heretofore been advanced to explain the highly interesting phenomena of that curious disorder, essential paroxysmal tachycardia. Perhaps some day our means of investigation will permit of a demonstration of changes, concerning which we can now only speculate.

The diagnosis of the affection is under ordinary circumstances attended with

but little difficulty. We primarily exclude all cases of permanent tachycardia, and then those cases which arise reflexly from disturbance or disease of any of the bodily organs. In this connection we remember that many cases arise from reflex causes of very obscure nature. But the symptom-complex of the disorder is so characteristic, that with a knowledge of the existence or possible presence of the affection, coupled with the otherwise negative result of an examination of the patient, one could hardly mistake it for anything else. We will further be careful not to confound the cases under consideration with those of acute and chronic angio-spastic dilatation of the heart to which Jacob[28] has lately directed attention. This is a disorder in which a general angiospasm, accompanied by chill, leads to excessively frequent heart-beat and subsequent dilatation of the heart. In essential paroxysmal tachycardia there are no symptoms of angiospasm. The diagnostic features of the affection, however, have already been sufficiently discussed. We must not neglect to bear in mind that well-described symptom-complex, to which Cohen has given the name vaso-motor ataxia, one of the features of which may be tachycardia.

The prognosis of the affection refers to the possibility of a cure being effected and to the danger attending the individual paroxysms. The obscure nature of many of the causes engendering attacks of tachycardia bestows an added impulse to the search for such cause in the hope that the disorder may be reflex, under which circumstances it will cease with the removal of the inducing cause. The prognosis of essential paroxysmal tachycardia is guardedly unfavorable as to cure. Some cases are subject to spontaneous cure; others endure for a long number of years, occasioning the patient but slight discomfort; while in some instances death has occurred during a paroxysm. We cannot presage the duration of either a paroxysm or the disease.

The treatment of the disorder is best comprised in the term symptomatic. In some instances, the measures that we employ symptomatically to alleviate the paroxysm have been followed by a permanent cessation of the disorder. It is well to remember that paroxysms have been controlled by pressure on the vagus in the neck, by electric stimulation of the vagus in the neck, by pressure over an ovary, by the drinking of cold water or of hot coffee, by deep inspiration and retention of the air. Of great value further are the application of an ice-bag to the chest and the swallowing of small bits of cracked ice; or a cold douche may be applied to the chest, abdomen or neck. Attacks of long duration are frequently favorably influenced by hypodermic injection of morphin. It is, however, worse than folly to resort to such a measure for the relief of attacks which, in all probability, will cease spontaneously in a few moments. Digitalis is at times resorted to, and its use is thought to be followed by satisfactory results. Various nervines, such as valerian, asafetida and zinc, have been recommended, but they are of doubtful utility. The methodic use of galvanization of the vagus in the neck has its supporters. Much more permanent effects, however, may be anticipated from strict attention to hygienic measures, the avoidance of excessive exertion, good diet and particular regard for the general nutrition and welfare of the body.

We have thus referred to an affection whose very name implies ignorance of its real nature, whose clinical manifestations are most interesting and for the explanation of whose phenomena we have suggested an adaptation of a lately advanced and alluring theory.

The cases reported were observed in the private practice of my father, Dr. Joseph V. Kelly, to whom I am indebted for the opportunities of seeing the patients, for some of the notes and for many kind suggestions.

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DISCUSSION.

Dr. A. A. Eshner related that in a considerable number of cases of vaso-motor ataxia in which he had examined the urine for Dr. S. S. Cohen, in some of which there had been rapidity of action of the heart, erythrocytes in varying number were found in a not very small proportion. The simplest explanation of the fact is that the relaxation of the renal vessels from diminished vaso-motor tonus permits of the escape of individual red corpuscles.

Dr. S. Solis-Cohen said that the cases to which Dr. Eshner referred did not number among them any that would fall under the definition of essential tachycardia in the sense that all other pathologic phenomena were absent from the complexus. He could not agree with the nosologic scheme that would sharply separate cases such as those reported from all other cases of cardiac and vaso-motor disturbance.

When there exists a large group of pathologic phenomena comprising cases that shade into one another imperceptibly, it seems a philosophic blunder to draw a sharp line at any one point arbitrarily chosen, saying cases on this side belong in one group, and cases on that side in another. This rather narrows our conception of the fundamental pathologic factors; whereas, if we include in our study all the cases—just as we would include in our study of zoology all the different varieties of a species, or all the different species of a genus—we are the more likely to arrive at a satisfactory conception of the causative mechanism. In other words, tachycardia is to be looked upon not as a disease, but as a symptom, just as glycosuria, albuminuria, asthma, and other phenomena formerly considered diseases are now looked upon as but symptoms.

Dr. Kelly's adaptation of the theory of the motility of the neuron to these cases is certainly ingenious, but the question at once comes up, Why is the contact of the arborescent extremities of the neurons interfered with? In the cases of vaso-motor disturbance studied by Dr. Cohen there seemed to be several exciting causes of the interference with vaso-motor and cardiac tone. This loss of tone was called *ataxia* because it is not a

paresis, and, though aggravated paroxysmally, it is not a spasm ; it is not an overstimulation, it is not an understimulation ; it is simply an irregularity. Less is assumed in terming it an ataxia than by using any other term.

In some, over-exertion was the exciting cause ; in some, emotion ; in some, change of temperature ; but in the majority of instances it was poisoning of some kind ; in some cases, poisoning by drugs, in other cases, poisoning by food, in other cases, poisoning by failure of function, intestinal or renal or thyroid, or other similar condition. We have thus advanced one step further if we say that the paroxysm is usually a toxic manifestation ; the toxins being many in number and various in character. These interfering with the function of the neuron, then the other conditions may occur. We must, however, go still further, and assume, as the condition rendering patients susceptible to this result of intoxication, a fundamental fault in the physical, chemic, or vital constitution of the nervous centers of visceral life, a failure of development that may be congenital, indeed hereditary, as it was in many of Dr. Cohen's cases, or similarly faulty constitution acquired as the result of various depressing influences, perhaps infectious disease, as in Dr. Kelly's case following influenza. The interference with the nutrition and with the further development of the nerves and ganglionic centers owing to the profoundly depressant effect of the poison of influenza was in that case equivalent to what would have occurred in some other cases through hereditary failure of development.

It is true that we have not yet reached the ultimate cause, but if we can go but these two steps further toward it we shall be better able to diagnosticate and to treat cases, and better able to formulate a prognosis in the individual case.

Dr. Cohen gave an account of a patient presenting a most marked case of vaso-motor ataxia. For six months he had been without any of the paroxysms to which he had been subject before coming under treatment, and he attributed the recurrence of the paroxysm to ale-drinking, which had shortly preceded it. This conclusion seemed justified, as errors in diet, drink, or venery usually preceded the paroxysms. The attack begins with blurring of vision, one-sided headache, as in ordinary migraine, but it then goes on to violent tachycardia, following which comes numbness of one upper extremity—always that opposite to the side of the head which is affected. The patient's mother had goiter ; he himself presented moderate enlargement of the thyroid gland, and was benefited by the use of thyroid extract in very small doses. This patient has been relieved in all of his paroxysms by taking nitroglycerin. In the attack which he reported, he took ten drops of the spirit of glonoin, with relief in two hours ; whereas, before treatment the condition required several days for complete relief. In this case the phenomena, as shown by facial pallor, are not those of paresis, but of over-constriction of the vessels, and nevertheless it is attended with tachycardia. In a case of paroxysmal tachycardia, observed by Dr. Cohen, the tachycardiac attacks seemed to alternate with ordinary paroxysms of epilepsy, and sometimes flushing of the face and tachycardia preceded or accompanied the convulsions. Dr. Wood has advanced the idea that there is a very intimate relationship between tachycardia and epilepsy, suggesting that tachycardia might really be called an epilepsy of the heart ; but the paroxysm and its provocative cause must be separated in study from the underlying susceptibility to such cause and such reaction before useful conclusions as to the true character of either can be reached.

Dr. Wharton Sinkler said that the occurrence of tachycardia in neurasthenics is so common that we cannot help associating the conditions which bring about neurasthenia with the attacks of tachycardia. The excessive heart-action is one of the most annoying symptoms in neurasthenics, and one of the most difficult to overcome. The theory of the motility of the neuron, while very attractive and plausible, is not based upon any firm foundation, and is purely speculative. There is no question that the neuron is an entity, and that disturbances in its function bring about disturbances throughout the nervous system.

It seems that the cause of disturbance which takes place is rather one of mal-nutrition than toxic, because neurasthenia is essentially a condition brought about by malnutrition, and improvement in the nutrition of the patient removes the neurasthenic state and along with that the attacks of tachycardia.

Dr. J. P. Crozer Griffith said that a survey of the subject had more and more impressed him with the impossibility of drawing sharp lines between so-called essential tachycardia and tachycardia from other causes. In many other affections it has become necessary to limit the use of the word "essential" very largely; for instance what was formerly called "essential anemia" we now attribute to a number of causes. The same facts apply to "essential epilepsy." Experiments of recent years have demonstrated distinct organic affections in many cases which for want of a better term were spoken of as "essential."

Attacks of essential tachycardia are simply those in which an originating influence cannot be found. The reflex tachycardias are naturally functional in origin.

Tachycardia is of frequent occurrence at the menopause, in conjunction with other disturbances. Dr. Griffith related the case of a woman in whom following a dislocation of the shoulder the heart remained for several weeks excessively rapid, with the pulse at 150, but curiously enough without any subjective

symptom whatever, aside from palpitation. Absolutely no organic cause for the condition was to be found, and the heart was perfectly normal.

Another case would have been classed at one time as essential tachycardia and at another as arrhythmia, which would suggest the existence of organic trouble, probably myocarditis.

The President said that he had recently seen two cases of so-called essential tachycardia. One occurred in a man who had endured great hardships in consequence of shipwreck. The other was in an elderly lady who had broken down after a prolonged period of anxiety due to the illness of her only daughter, followed by serious pecuniary loss. Both cases presented in a typical manner the phenomena of essential tachycardia. There were, in both, intervals of prolonged relief from cardiac symptoms, with paroxysms occurring with great suddenness and usually without obvious immediate cause. Dr. Wilson coincides in the view that even so-called essential tachycardia must be looked upon as symptomatic, as it is often associated with conditions of malnutrition which, for want of a better term, are classed under neurasthenia.

Dr. T. J. Mays expressed the conviction that some cases of tachycardia will show organic changes in some portion of the nervous system. Cases of essential paroxysmal tachycardia are always puzzling because they do not harmonize with views gained from physiology. He concurred in the view that often in these cases the accelerator nerves are concerned, while the vagus or the medulla oblongata is involved principally.

The literature contains a number of cases in which pressure by an enlarged tumor or gland upon the vagus produced tachycardia, showing that knowledge of the normal inhibitory power of the vagus does not harmonize with pathologic knowledge of this condition. If it did then the heart's action would be slow instead of accelerated in these cases. The possible explanation offers itself, however, in this connection, that the pressure on the vagus may paralyze its inhibitory power, and that the heart's action is more dominated by the accelerator nerves. The belief was expressed that the condition of paroxysmal tachycardia is associated with many abnormal conditions, and that it is always a condition of depression. Dr. Mays has found it associated with angina pectoris and believed that it is a spasmodic disease and has a close alliance with epilepsy and probably with asthma too. Furthermore, he has found it in cases of lead-poisoning. It is also found in alcoholic poisoning and also in syphilis, and possibly in some other metallic toxic conditions.

No one plan of therapeutics is adapted for the condition, which requires a varied application of remedies. In one case of angina pectoris, associated with the attacks, applications of ice to the neck proved very effective.

Most cases are benefited by strychnin, together with general supportive treatment, including nourishing food, and, if well-borne, cod-liver oil. To give these patients the best therapeutic results it is necessary to operate through the whole nervous system.

Dr. A. O. J. Kelly agreed that taking any series of cases as they present themselves, tachycardia included, it is absolutely impossible to arrange them all in any exact nosologic order. Nevertheless, it is quite true that the cases reported, especially the first two, are instances of what may be properly designated essential paroxysmal tachycardia, according to the definition given at the beginning of the paper.

There are many cases in which the ultimate principles occasioning the tachycardiac symptoms are toxic. These cases may be designated symptomatic or reflex; some of them are alcoholic. It is interesting in this connection to note that Déjérine, in some such cases, has demonstrated, after death, degeneration of some of the fibers of the vagus. In elaborating the theory of the motility of the neuron it is admitted that the ultimate principle might be in many instances nutritional.

The best treatment may be termed symptomatic, and it is well to remember that numerous paroxysms may be terminated by stimulation of the vagus in the neck, and by such procedures as drinking a cup of cold water or hot coffee. An ice-bag to the precordium or a cold douche to the chest or neck may be adequate.

Under certain circumstances the injection of morphin is followed by good results, but certainly in minor paroxysms of tachycardia this drug is not indicated. In prolonged attacks it has been given with asserted good results. In other cases digitalis has been given and the attacks have ceased. The result, however, cannot always be ascribed to the medication employed.

THE BLOOD IN CHOREA.*

CHARLES W. BURR, M.D.

[Abstract of article read October 14, 1896.]

The blood of thirty-six cases of Sydenham's chorea, treated in the clinics of Drs. Weir Mitchell, Sinkler and Lewis, at the Infirmary for Nervous Diseases, was examined by Dr. James Ely Talley. In each case the hemoglobin was estimated and the red corpuscles counted. No count was made of the white corpuscles. The hemoglobin and the corpuscles were at the theoretic normal in only two cases. In none was the anemia extreme. In the most severe case, the hemoglobin-estimation was forty-five per cent., and the number of red corpuscles 3,450,000. The chorea was slight and the patient had serious heart-disease, to which the anemia was referred. In another case, complicated by epilepsy, the hemoglobin-estimation was sixty per cent., and the number of red corpuscles 3,600,000. In one very bad, uncomplicated case, the hemoglobin-estimation was sixty-five per cent. and the number of red corpuscles 4,800,000, and in another the hemoglobin-estimation was sixty per cent. and the number of red corpuscles 3,850,000. These were the most anemic cases. In twenty-four, the hemoglobin-estimation reached eighty per cent. or over, and the number of corpuscles 4,000,000 or more. In twenty-nine the hemoglobin-estimation reached seventy-five per cent. or over.

The conclusion was reached that the blood is rarely absolutely normal during an attack of chorea. There is usually a moderate diminution in the hemoglobin and a relatively still smaller decrease in the number of red corpuscles. The anemia is, therefore, chlorotic in type. There is no relation between the severity of the chorea and the severity of the anemia. When the anemia is severe, there is usually some complication competent to explain it. The belief was expressed that anemia is not an immediate, direct, exciting cause of chorea, and but infrequently a predisposing cause. In the great mass of choreas, the anemia is the result of the chorea—is secondary. In the rare cases of chorea, which seem to be caused by mycotic infection, the anemia may be toxic.

DISCUSSION.

Dr. Wharton Sinkler agreed with all that Dr. Burr had said. It is the popular impression—that which prevails among the medical profession at large—that anemia is a predisposing cause of chorea. The Collective Investigation Committee of the British Medical Association a few years ago collected 432 cases of chorea and found that ninety-two of these were anemic. Dr. Rachford, of Cincinnati, considers that chorea is due to what he calls a scrofulous anemia, and says that it is directly curable by iron iodid.

From his own experience, Dr. Sinkler does not think that in Philadelphia, or at least among the patients who come to the Infirmary for Nervous Diseases, choreic children as a rule look anemic. A scrutiny of the books at the Infirmary shows that of forty-six cases in

which a blood-count was made—probably including those studied by Dr. Burr—the average number of corpuscles was 4,825,000 to the cubic millimeter; in seven cases the number of corpuscles amounted to 6,000,000 and over; in ten, between 5,000,000 and 6,000,000; and in sixteen between 4,500,000 and 5,000,000. The hemoglobin-estimations were practically normal—that is, out of forty-seven cases in which an examination was made as to the percentage of hemoglobin there was an average of 79.4 per cent. The lowest blood-count was 2,800,000; the highest, 6,500,000. In twelve cases the hemoglobin was estimated at over 90 per cent. These results are about as good as would be expected in an equal number of healthy children. They bear out the assertion that anemia does not predispose to chorea.

Dr. Alfred Stengel said that his experience in the examination of the blood in cases of chorea is limited to about a dozen cases. In these the average blood-count was between 4,000,000 and 5,000,000, and on the average, the hemoglobin was about equally reduced. In no case was there marked anemia. In a few cases there was a moderate leukocytosis. Altogether, the conclusion seems justified that anemia is not a frequent complication of chorea, much less a cause of the disease.

The interesting fact, however, to be considered is that many cases of chorea present an anemic appearance. This has led to the general belief that chorea is associated with anemia and is probably dependent upon it. An anemic appearance, however, is by no means always an indication of actual anemia. In a recent paper on chlorosis, by Dr. Townsend, of Boston, allusion is made to the unreliability of pallor of the skin or mucous membranes as a sign of reduction of hemoglobin. The reverse is also true. Individuals who have normal blood may present an exceedingly anemic appearance.

A few years ago a form of disease was described by Winternitz, of Vienna, as *anæmia spuria acutissima*. A female servant who, in trying to mount a ladder, fell, suffered a severe nervous shock, and at once became extremely pallid. The pallor persisted, but on examination the blood was found entirely normal. This condition was believed to be due to disturbance of the vaso-motor system. Perhaps the same principle may apply to chorea and other nervous diseases.

The possibility of there being quantitative anemia in chorea is, of course, a mere theory, as there is no way by which the presence of quantitative anemia during life, or even after death, can be reliably determined. However, all the associated conditions and the experimental evidence point to the fact that quantitative anemia, if it ever exists, is very rare. There may sometimes be such a thing in progressive pernicious anemia. Dr. Stengel alluded to a case in which there was an extremely anemic and desiccated condition of the organs, in which the blood-vessels throughout the entire system were empty of blood, and in which the blood-count had been very low. Such cases are possibly instances of quantitative anemia, but in chorea the appearances are never of this character. There is not the same difficulty in obtaining blood from the finger; not the shriveled or shrunken condition of the tissues; not the apparent failure of the peripheral circulation present in cases displaying the external appearances of what would be expected of a quantitative anemia.

It is to be believed, therefore, that in cases of chorea there is only that amount of anemia which comes from some toxic or some general nervous disturbance. Chorea may be looked upon as a toxic condition, probably an infectious toxemia, and the existence of a moderate leukocytosis, as found in some cases, may or may not be taken as of some significance in this connection. Apart from the actual anemia, the spurious anemia or pseudo-anemia so often met with must not be ignored, and this for want of a better explanation may be ascribed to vaso-motor conditions. As many of the blood-examinations in cases of chorea are made in out-door clinics, and most of the patients are young persons, the examinations are in themselves unreliable to some extent. They are fallacious from the defects of the instruments used; and, furthermore, the excitation of the heart and of the circulation, consequent upon coming to the clinic, causes a peripheral corpuscular congestion (if such a term may be used), from the more vigorous action of the heart in driving blood into the periphery; or there may be the reverse condition due to cold, with contraction of the blood-vessels, which raises the blood-count by causing venous congestion. Therefore, there is probably a higher degree of anemia than appears in the examination, but decided anemia is unquestionably very infrequent in chorea.

Dr. James Tyson said that the entire compatibility of pallor with a normal composition of the blood is evident when we think of the pallor that attends the syncopal attack, which furnishes for a short time all the external phenomena of anemia. It would be an interesting matter to determine the condition of the heart in these cases in which there is an appearance of anemia in association with chorea while the actual blood-count and hemoglobin-measurement fail to disclose the essential conditions of anemia. We have only to consider a permanence of the conditions that prevail during a fainting fit, of course in a milder degree than in an actual faint, and we have those of a chorea and yet a normal composition of the blood.

Dr. Wharton Sinkler asked for an expression of opinion from those who have had experience in the matter of blood-counting and estimation of hemoglobin as to how much

the results of examinations depend on the individual.

Dr. Alfred Stengel said that he had made no study of the errors in blood-counting, but in a recent paper by Moyer (*Deutsches Archiv für Klinische Medicin*, Bd. lxvi) upon the errors of the hemometer of Fleischl, variations in results were attributed to fatigue, the personal equation, and the skill of the experimenter, as well as to defects in the instrument itself. In examinations made simultaneously by Dr. Stengel and a colleague some years ago with the Fleischl instrument a difference of ten per cent. was sometimes found. Gradually with increasing experience the error decreased.

SURGERY FOR TYPHOID PERFORATIONS.

JOSEPH PRICE, M.D.

[Read October 28, 1896.]¶

I submit the following report of cases operated on for typhoid perforation for the lessons they may convey, and will discuss them from the standpoint of our more recent experience.

Case 1.—Mrs. A. O., aged thirty, having several children, but without a history of miscarriages, was admitted to the hospital on October 2, 1896. She was seen by Drs. Hughes and Owen in consultation after some three weeks of illness, with a typical history of typhoid fever. Operation was performed on October 1st. Symptoms of perforation were present, with well-localized attacks of peritonitis and an irregular and ill-defined tumor on the right side. Omentum and small bowel were found adherent in the region of the ileo-cecal valve. The adhesions were easily freed and two perforating ulcers, six inches apart, were found. The lower one, situated a few inches from the valve, was large, irregular, and necrotic; the second one was higher up in the bowel, about one-half inch in length, well defined, and less healthy in appearance.

A puddle of filthy fluid was found about the perforations, and the omentum and appendix were also involved in the adhesions. The infected portions of the omentum and appendix were removed. The holes in the ileum were trimmed and sutured, and an irrigation-toilet was followed by both glass and gauze drainage. The mesenteric and retroperitoneal lymphatics were generally enlarged and easily recognized by touch and sight. Recovery ensued without a hitch.

Case 2.—Mrs. B. K., a married woman, aged twenty-six, with two children and a history of one miscarriage, was admitted to the hospital on June 4, 1896. She had a rapid pulse and high temperature, and appeared to be in a decided septic condition. Peritonitis was quite general, and alarming emaciation had taken place, apparently as a result of some lung-trouble. Section was made on June 5th. General adhesions were found in the region of the ileum and right groin. When all adhesions had been freed, a large, ragged perforation was found in the ileum, with a circumscribed accumulation of bowel-contents. The perforation was trimmed and sutured. After an irrigation-toilet, glass and gauze drainage was provided. Recovery followed. For two days following the operation the pulse remained high and feeble. The temperature also was high, and this was considered rather favorable. This patient had been very ill for two weeks before admission to the hospital. The character of the ulceration was doubtful, as there was tuberculous trouble in the lungs. The closure of the fistula after suturing is rather against tubercle, as tuberculous fistulæ rarely close by suture.

Case 3.—Mrs. R. B., a married woman, aged twenty-eight, without children and without a history of miscarriages, came under observation after three weeks of illness and treatment for typhoid fever. She was admitted to the hospital on January 5, 1895, and went into collapse soon afterward, being unconscious at the time of operation. On January 6th section was undertaken, freeing all ad-

hesions, stitching multiple bowel-fistulæ, detaching lymph from the bowel with gauze. There was a general angry peritonitis, with filthy bowel-contents, and filthy inflammatory products throughout the peritoneal cavity. Gaseous distention was marked and the peritoneum had a decided fecal odor. Thorough irrigation and drainage were practised. I never attempted to close a filthier peritoneal cavity than this, either ante-mortem or post-mortem. Recovery followed. There were present at the operation Dr. N. Fred Essig, of Spokane, Washington; Dr. Samuel S. Q. Robinson, of the U. S. Army; Dr. Harold Bunn, of California; Dr. John F. Roeder, Dr. H. S. Lewers, and Dr. Garden, of Philadelphia.

Notwithstanding the great progress medical and surgical science has made, typhoid fever continues to present many complex and difficult questions. It must be classed among the most grave troubles with which the profession has to deal. Little is known about the disease, other than of its more objective symptoms. There is no exactitude or certainty in its treatment, which is rarely the same by any two physicians. The treatment begins with guesses and grows into some degree of certainty only as conditions improve. I will not attempt to deal with the larger circle of facts connected with typhoid fever, but will restrict my discussion to the surgical treatment of typhoid perforation.

Again, we have a wide divergence of opinion as to the propriety or wisdom of operation. There is no very general accord of opinion as to prognosis or the definiteness and reliability of symptoms —as to reliable evidence of perforation —nor is it agreed that all these cases prove fatal.

Dr. Reginald H. Fitz, of Boston, has furnished valuable data as the result of a study of the work of the earlier investigators as to the fatality of typhoid perforation. Louis, Chomel, and Jenner have reported numerous cases of typhoid perforation, but none of recovery. Tweedle says: "Intestinal perforation is always fatal, generally within thirty-six hours." Some more recent authorities make more favorable reports, others agreeing with the earlier authorities as to the almost certain fatality.

Griesinger holds that there is a possibility of the healing of a perforation and of recovery "never in cases of general peritonitis, only when the inflammation is wholly circumscribed. The rare exceptions are hardly worth considering in connection with the prognosis, which is to be regarded as almost fatal when the symptoms of perforation are distinct, and as absolutely fatal when gas is present over the liver. Murchison, who has contributed much that is valuable to the literature of the subject, says that "rare cases are met with where recovery ensues after all the symptoms of peritonitis from perforation." Dr. Reeves reports that: "I have seen in five instances all the symptoms which announce and follow perforation of the bowels, yet the patients recovered." Dr. Loomis, in discussing the subject, says: "I do not remember to have seen a single recovery after there were unmistakable evidences of intestinal perforation. Recovery from a local peritonitis complicating typhoid fever is not uncommon, but when the characteristic symptoms of intestinal perforation are present, in my experience, a fatal issue soon follows." So we have the weight of authority on the side of almost certain fatality.

In the reported cases, due allowance must be made for errors of diagnosis. In many of these cases the diagnosis was not made until post-mortem examination revealed the characteristic typhoid lesions. Had recovery taken place, much doubt would have remained in the mind of the operator as to the real nature of the perforation. We know that typhoid perforations are the most common variety of perforations, and the perforation is usually in the ileum.

As to the mortality in cases of perforation of the bowel, Dr. Osler gives recent statistics: "In 114 cases of the 2000 Munich autopsies (5.7 per cent.) and in fourteen instances in my series, the intestine was perforated and death caused by peritonitis. The perforation may occur in ulcers from which the sloughs have already separated, or it

may be directly due to the extension of a necrosis through all the coats. In only a few cases is the perforation at the bottom of a clean, thin-walled ulcer. In one instance the perforation occurred two weeks after the temperature had become normal. The sloughs were, as a rule, adherent about the site of the perforation. A majority of the cases were in small, deep ulcers. There may be two or even three perforations. The orifice is usually within the last foot of the ileum. In only one of my cases was it distant eighteen inches. Peritonitis was present in every instance.

"Hemorrhage from the bowels occurred in ninety-nine of the Munich cases, and in nine of my series. The bleeding seems to result directly from the separation of the sloughs. I was not able in any instance to find the bleeding vessel. In one case only a single patch had sloughed, and a firm clot was adherent to it. The bleeding may also come from the soft, swollen edges of the patch. Peritonitis without perforation may also occur by extension from the ulcer, or, occasionally, by rupture of a softened mesenteric gland. It was present in 2.2 per cent. of the Munich autopsies."

The question is direct, What chances does surgery offer? The one and only chance left. We know the almost inevitable sequel in one case and something of the possibilities in the other. The one means death, the other gives a chance of recovery. The error, to put it mildly, consists in abandoning these cases as absolutely hopeless, when there is yet one last resort,—surgery,—which furnishes precedents of encouraging success. I am not venturing upon entirely new ground. Dr. James C. Wilson, the honored President of this society, a clinician of wide experience, stands among the first, if not the first, to advocate, in clear, unequivocal language, surgical dealing with these cases. Dr. Hunter McGuire, of Richmond, Va., a worthy supporter of the fame of the old school of surgeons, recommends the tying of vessels to control hemorrhage from ulcers in typhoid fever. He recognizes that too many are lost from this cause, and suggests an original and ingenious method of suturing to control the hemorrhage and avoid necrosis. We are slow in following the lead these men take, slow and hesitating in adopting their urgent suggestions, in coming down from our theoretic lofty height.

All our surgical procedures have made their way in the face of relentless criticism and opposition. Surgical interference, in cases of typhoid perforation, has not proved an exception. Largely, the difficulty lies in timidity and oversensitiveness as to professional repute. The protective character of adhesions is often misleading, tending to lull apprehension as to immediate existing risks to life. The condition is too frequently classified for non-interference—left to the processes of nature—when parts are weakened and poisoned beyond the kindly healing and remedial processes of nature. We find, occasionally, recorded deaths from spontaneous perforation due to chronic local peritonitis. The history may be that of localized attacks of peritonitis—with doubtful evidence of perforation—the localized attack resulting simply in adhesions about the ulcer. If the adhesions are well formed the escape of gas and bowel-contents will be limited when perforation occurs.

The patching or fortification by adhesive and protective peritonitis, avoiding acute general peritonitis and sepsis, gives us the most favorable class of cases for surgery. Localized peritonitis, with adhesions, with or without perforation, around an ulcer, with sufficient adhesive and inflammatory product to form a small tumor, is quite easily recognizable in an emaciated patient. An eminent surgeon says, in connection with these cases, that which cannot be accepted as safe dictum:

"Surgeons are not justified in performing laparotomy for the suturing of perforated typhoid ulcers, if circumscribed peritonitis of an adhesive or protective character exist, or is in process of development."

The trouble, as with all intestinal affections, is a hidden one, not one directly addressed to our vision. We cannot determine with any large degree of certainty, even from a few marked

objective signs, the extent of the protective character of the adhesions, nor determine anything certain as to the character or extent of the process of development. We know the sequence in the majority of these cases when there is no interference. Perforations or fistulæ, due to ulceration and sloughing, rarely close. Almost all such ulcers are surrounded by adhesions, with pus, bowel-contents, fistulæ and fistulous openings. Complications become general, keeping the patient in a miserable condition; emaciated and anxious, with a rapid pulse, cold, clammy and greatly wasted. Fistulæ of viscera, due to incision or surgery, commonly close spontaneously. Not so, however, when due to sloughing. Unfortunately, we are not always aided by the clinical history in our diagnosis. We are directed or guided largely by the patient's general condition, the peritonitis or the small and ill-defined tumor.

There is but little difficulty in settling the fact that the patient is dying of some intra-peritoneal lesion. Errors are rarely made in opening the abdomen. Suture-methods of repair, after careful trimming of the ulceration, give the most pleasing results. Excisions or resections have nothing to recommend them. The open treatment, when the conditions are desperate, and sepsis and bowel-distention very marked, favors peritoneal and bowel drainage of all contents. An abundance of gauze placed about the fistula in the shape of a square coffer-dam favors simple drainage and avoids contamination. The large mortality has been largely due to clumsy and imperfect work. Everything within the abdomen is intolerant of bungling manipulation. The surgery is not to be gone at with that awkwardness with which a man would try to put his five fingers in a glove with four. The delicacy of the condition of the parts, which the very nature of the disease creates, requires in the surgery the use of fingers delicate and sensitive of touch and deft in use.

The repair of perforations, commonly single, rarely multiple, is easy and should be rapid. There may be some delay in seeking and finding the point of perforation, but the well-defined nature of the pathologic condition at that point is easily recognized by fingers familiar with normal intra-peritoneal conditions. The deviation from the normal can be instantly recognized when the fingers are passed through the viscera, without exposure. The cluster of adhesions, omentum and bowel about the perforation are easily freed. The cleansing, local and general toilet, are of great importance. Rarely do we find distention associated with perforations, except in the delayed cases, on the third or fourth day after perforation.

In delayed cases the mass is well marked; paresis of the bowel, with over-distention, is prominent. The characteristic fecal odor is recognized at once upon opening the abdomen. This is most marked in the acute cases, in those dying soon after perforation. If the adhesions are well formed about the perforation, a fecal odor is rarely present. When patients are under observation, the diagnosis made early, the disease running a uniform course, with a definite train of symptoms, the characteristic morning remissions and evening exacerbations, and about the third week a copious intestinal hemorrhage takes place, with the patient sinking into fatal collapse, with a quick pulse, subnormal temperature, the symptoms admit of but one interpretation, and point to but one possible source of relief.

In the very nature of things, from the very character of the trouble and the parts attacked, the mortality will always be large; but some can be saved. The stimulus of anesthesia increases the force of the pulse, the patient's respirations deepen, and at the completion of many of these operations the patient's general condition is often better than before. An irrigation-toilet, aside from having great value for cleansing, is a stimulant to the solar plexus and favors reaction.

The same principles apply in these cases of typhoid perforating ulcers that apply in cases of general septic or purulent peritonitis and to stab-wounds and gun-shot wounds. The words of the late Dr. D. Hayes Agnew, who, in his day, was the sovereign spirit of American sur-

gery, as applied by him to gun-shot wounds of the abdomen, apply with equal appropriateness to typhoid perforations. He says: "I want to place myself upon record, for I have very strong convictions with regard to laparotomy. They amount to this: If there is a reasonable degree of evidence that there is a penetrating abdominal wound, especially if a shot-wound, it is our duty to open the abdomen, to make an exploratory incision. We are not to be deterred by the possibility of some legal technicality, if the case should come into court. We are to do our duty without reference to consequences."

I will quote extensively from Dr. J. C. Wilson, for nothing better has been said upon the subject:

"I take it for granted that almost every case of free extravasation of intestinal contents, however small in amount, into the peritoneal cavity terminates fatally. There is little reason to believe that any case of this kind recovers. It is important to note that the cases of peritonitis in enteric fever in which recovery is possible can be clinically distinguished from those which will terminate rapidly in death. The clinical picture of the two conditions is almost as distinct as are the pathological lesions. Where there is extravasation of the intestinal contents into the peritoneal cavity, the collapse is like that caused by the escape of an amount of foreign matter, the result of a perforating gun-shot wound of the intestine. The proposition which I submit for discussion arises directly from a consideration of the matter in this way. Until within a few years, no surgeon realized the possibility of treating cases of gun-shot injury of the abdomen with perforation of the intestine and the escape of blood and fecal matter by the operation of laparotomy, washing out the peritoneal cavity, excising bruised and lacerated portions of the intestine, and bringing the parts together by suture. Yet this is now the recognized procedure in such cases, and has been of late practised in many instances with success in cases that, under the old plan of opium and expectancy, would have inevitably perished.

"Are we ready to adopt the same measures in perforation of the intestine with similar conditions as regards the peritoneal cavity, and a like helplessness as regards cure by opium and expectancy in our cases of enteric fever? Recognizing the two groups of cases I have described, and being, as we are, able to refer almost all cases to either one or the other of them within a few hours of the development of the symptoms, are we prepared to decide—and to do so with the necessary promptness—upon those operative procedures by which alone in the second group the life of the patient may be saved.

"Granted that the chances of a successful issue are heavily against you; that the patient is in the midst or at the end of a long sickness; that his tissues are in the worst state to stand the injuries of the surgeon's knife; that the lesions of the gut may be very extensive; that the vital forces are at the lowest ebb. No one yet has hesitated to perform tracheotomy in the laryngeal complications of enteric fever, which require it to save life, for these reasons.

"The operative treatment of purulent peritonitis has been performed many times successfully by the gynecologist in conditions scarcely less unpromising. In point of fact, the objections that may be urged against laparotomy in intestinal perforation in enteric fever are no more forcible than those which would have been made use of at first against the same operation in gun-shot wounds of the abdomen. The courage to perform it will come of the knowledge that the only alternative is the patient's death."

Dr. Wilson, with his advanced, pioneer views in this connection, does not furnish the first illustration of the physician taking the lead of the surgeon, furnishing the guiding, the impelling thought, not infrequently the courage. About seven years ago the American Surgical Association and the Association of American Physicians discussed, at the same time and in the same building, the relative merits of surgical and non-surgical interference in appendicitis, the medical body deciding in favor of prompt operative interference, the

surgeons for delay. Almost coincident with Dr. Wilson's advocacy of celiotomy for the relief of intestinal perforation in typhoid fever, Dr. Lewis S. McMurtry, of Louisville, Ky., performed an operation, the subject being a physician, and found multiple perforations. He trimmed the holes, closed them with sutures, irrigated and drained, recovery following. A report of this case, with the patient present at the time, was made at the Cincinnati meeting of the American Medical Association.

There is another recorded case—that of McArdle, of Dublin. The history is one of abscess and multiple perforations following an accident, occasioned by jumping from a wagon. The accident is not a very satisfactory explanation of the trouble in this case. The evidence better supports the conclusion that the case was one of walking typhoid fever with multiple perforations. I might refer to cases in my own experience and that of others, in which the history was doubtful. A considerable number of operations for circumscribed abscess have been reported as successful. Many of these cases are quite as questionable in their history as are those for which post-mortem operation has been done or refused.

In this connection Fitz says:

"Although the reported instances of the successful results of an operation for the cure of circumscribed peritonitis in typhoid fever are comparatively few, I have been able to collect a considerable number in which recovery resulted from resolution or from the spontaneous evacuation of the inflammatory product. In seventeen cases of recovery by resolution the peritonitic attack began in the second week in one, in the third week in eight, in the fourth week in one, in the fifth week in one, and in the sixth week in two. It began at the end of the fever in one, and during convalescence in three. Recovery took place in a week in one, in two weeks in three, in three weeks in two, in four weeks in one, and in two or three months in three. The length of time necessary for recovery in the remaining cases was not stated."

It is a mistake, on the part of gynecologists and obstetricians, to apply the term typhoid fever to certain septic conditions. The sponge-tent, the curet, the sound and a variety of minor gynecologic operations have been followed by septic conditions and abscesses, frequent pulse, high temperature and diarrhea—simulating typhoid fever.

Obstetricians are in the habit of reporting septic cases under the head of malaria. The recorded mortality is largely from the prolonged anesthesia of a patient already enfeebled and with a greatly weakened heart, and the great length of time taken in the operations. They will not stand prolonged anesthesia or a prolonged operation. In a large percentage of those dying after long anesthesia and operation, death is due to causes within the surgeon's control.

One of the common causes complained of is that of weak, unhealthy tissue, and the yielding of sutures. Herein lie two errors—the choice of needle and that of suture-material. The best needle is that from the woman's sewing-case—a fine, round needle, and 0 or 00 Chinese silk.

Early diagnosis, early operation, painstaking, rapid work will save many lives.

Courage goes hand in hand with reverence for human life. There is much force in what Napoleon said to Las Casas: "As to moral courage, I have rarely met with two o'clock-in-the-morning kind. I mean unprepared courage, that which is necessary on an unexpected occasion, and which, in spite of the most unforeseen events, leaves full freedom of judgment and decision." It is two o'clock-in-the-morning courage we need—the factor that goes largely to settle the result in many surgical cases for us is the lost quarter of an hour

DISCUSSION.

Dr. Geo. E. Shoemaker said that the time has come when the courage which it takes to operate in cases of typhoid perforation is to be rewarded. Granted that the diagnosis has been carefully made, it seems that the surgeon should not hesitate to intervene in this way with the object of saving an almost hopelessly lost life. Too great emphasis, however, cannot be placed upon the importance in this connection of rapid and deft surgery. The man who is unskilled in the best technic adapted to work of this kind should certainly not attempt this character of operation. The parts will not bear handling. No touch upon a peritoneal surface must be made unless it has a definite object to accomplish, and after this is accomplished the parts must not be meddled with. With rapid surgery and the knowledge obtained from dealing with septic peritonitis, which leads us to use gauze drainage in addition to glass drainage, the surgeon will certainly obtain some reward for efforts in this direction.

Dr. F. Woodbury said that Dr. Price is to be congratulated on his results, especially in view of the circumstance that these cases were brought from their own homes to the hospital in the third week of typhoid fever and with perforation and peritonitis, in what is generally regarded as a very serious condition, and one indeed in a state of collapse. Dr. Woodbury noticed years ago in hospital experience a long series of cases in which the patients with typhoid fever, who were removed to the hospital in the second or third week of the disease, were very likely to die. They did badly, and the disturbance and change of surroundings, and the nervous excitement attending the change at the height of the disease, were sufficient to seriously disturb the prognosis. Dr. Woodbury asked if, in a series of cases of the character under consideration, suffering from this grave complication of typhoid, the prognosis would not be somewhat improved by doing the operation at the patient's own home instead of transporting him or her to a hospital? And, in the second place, as to the indication for operation. May it be assumed in all cases of typhoid fever in which there is an appearance of local peritonitis, that a perforation has occurred and that celiotomy is indicated? A third question is: Can we obtain from the character of the discharges any guiding principle, or any assistance in making up our opinion as to the necessity for this operation?

For instance: in a case in which during the third week of typhoid fever the patient has committed some excess in diet, has eaten some hard substance, or taken some orange-juice and swallowed a seed, and perforation has resulted; knowing this fact, it would seem that the indication for operation would be imperative. Also, when the discharges from the bowel are very fetid and may be assumed to be highly toxic in character, their presence in the peritoneal cavity would be a powerful argument in favor of operating.

On the contrary, if, in the treatment of the disease, the bowel-contents have been kept in as nearly an aseptic condition as possible by the administration of some such agent as B-naphthol, or naphthalin or guaiacol, so that the discharges are kept almost odorless, and when there is otherwise doubt as to the necessity of operation, would this fact throw the balance in favor of delay or not?

Dr. W. E. Hughes said that, as a physician, he was perhaps a little more ready to recommend operation than a surgeon might be, but it is unquestionable (certainly from his experience) that a pronounced perforation in the course of typhoid fever means infallibly death, unless prevented by operative interference. He does not look on perforations in the course of typhoid fever as very largely mechanical, but thinks that there is usually preceding them a certain amount of local peritonitis. That is, the process has been virulent enough to infect a small area of peritoneum about the ulcer. Perforation in the course of typhoid fever, then, can be resolved into possibly two classes: one, in which the process is an exceedingly virulent one, and, after a preliminary very trifling peritonitis there is a large perforation, free escape of contents of the intestines into the abdominal cavity and necessarily widespread peritonitis. Cases of this kind die within a few hours or a day at most after perforation. There has usually been a well-marked history of typhoid fever, and there can be little mistake in diagnosis. In the other class of cases perforation has been more prolonged, or at least there have been primary adhesions preceding perforation, and here there may easily arise a very serious error. From past experience, Dr. Hughes would be inclined to recommend for operation not a case of simple peritonitis, but a case in which this peritonitis had become pronounced enough to be recognized as ushering in perforation, because then he would fear acute peritonitis, or after perforation had occurred that there would soon be peritonitis. The diagnosis of perforation in typhoid fever is exceedingly difficult in the walking cases in which no clear history of typhoid fever can be obtained, but in which there is unquestionably a local peritoneal condition, which would indicate operation, and whether the case be one of typhoid fever or of appendicitis.

Dr. Hughes related the history of a child some seven years of age, seen in consultation,

who presented a history of rather indefinite belly-pain, with some little irregularity of the bowels for ten days or so before the acute onset. The attending physician maintained that there had been no elevation of temperature whatever until three days previously to the visit. Then there was a rapid rise in temperature, with extreme distention of the belly, vomiting, and a great deal of pain. The child became wildly delirious, and appeared extremely ill.

When the child was first seen there was a grave suspicion of typhoid fever, but nothing to base a positive diagnosis upon. There was a little rigidity in the right iliac fossa, and a peculiar doughy feel of peritonitis, and operation was advised. The surgeon consulted refused to operate because the case looked so much like one of typhoid fever and minimized the local symptoms. On the following morning the belly had become soft, the temperature had fallen, and typhoid spots were visible. Two or three days later sudden collapse occurred, with death. Post-mortem examination showed a localized abscess following on perforation and then giving way of adhesions. The case was one of typhoid fever complicated with peritonitis.

In another case, admitted to the Presbyterian Hospital during the second week of unquestioned typhoid fever, suddenly the temperature rose, the pulse fell below normal, and there was a condition of collapse, with pain in the right iliac fossa, distended belly, appreciable rigidity in the region of the appendix, followed by vomiting, which became stercoraceous, and death finally ensuing. Post-mortem examination showed that there had been a localized abscess induced by peritonitis, with rupture of the abscess and general septic peritonitis. Nine days had elapsed between the origin of the abscess and death. It would probably have been easy to have saved that patient's life by means of an operation. Thus in the class of cases in which there is a slow oncoming of the perforation operation is unquestionably absolutely necessary, and will save a large percentage of lives, the more so as these cases are rather common in typhoid fever.

Dr. Hughes scarcely believed it possible to stop intestinal hemorrhage by any operative interference, or, if it be possible, it is surely not possible to tell in which cases operative interference should be instituted. In his experience, hemorrhage in typhoid fever is a bleeding from a number of ulcers much more frequently than it is of bleeding from any one. In the last two cases, dead from hemorrhage, in which he made post-mortem examinations, the bleeding had taken place from the colon, and not from the ileum. Such cases as these are not operable cases. When there has been a sudden single profuse hemorrhage, the site of bleeding islikely to be the ileum, while when there has been a series of small hemorrhages culminating finally in one large fatal one, it is more likely that the site of the bleeding is the colon.

Dr. Mordecai Price said that as a general thing he believed that when actual violence has been done to the bowel, the ulceration is of the character of simple impinging upon the peritoneum, and, as has been said, a patch has been applied, and if the leakage takes place it is confined to a small area of the peritoneum, and there is ample warning of the danger to the patient. In these cases we have almost assurance that surgery will be a saving of life.

In the other cases in which from some violence or straining, or from some foreign body that comes in contact with the ulcer and produces perforation, general peritonitis and death are likely to result in a very few hours, and unless the lesion is one that has been anticipated, the results of the operation, to be sure, are very doubtful, but surgery should be attempted in these cases.

Dr. Price contended that the danger of transporting cases to the hospital was not so great as that of failing to do so early enough. It would naturally be better to do operations at home if the patients were favorably situated. But the surroundings are often our masters. For instance, many cases to be operated upon come from hovels and without a clean towel or a clean basin in the house. In many cases it is necessary to supply not only the surgery, but also clean bed-linen and bed-clothing. For poverty-stricken patients requiring operation the hospital is unquestionably the best place. The surroundings are better, the nurses are better, the food is better, and the whole *morale* of the case is better.

Dr. J. B. Roberts said that no modern surgeon, educated in current methods, would hesitate to operate upon a case of perforation in typhoid fever. Of course there are surgeons who, perhaps, have not come quite to the modern standard in practice who might refuse, because they have not become quite convinced of the value of antisepsis and asepsis. The indications seem clear that in any sort of a perforation in the belly the abdomen ought to be opened and the perforation sewed up. The operation will, of course, very often prove fatal. It does not make any difference about statistics in these cases any more than it would in intraperitoneal rupture of the bladder. It is the surgeon's duty to do what he can as promptly as possible, and not to be deterred by statistics in surgery or any line of treatment. It is better in the majority of surgical cases to remove the patient to a hospital or some place where proper surroundings can be secured.

Dr. G. G. Davis said that the question of operation has been settled practically by the physicians themselves, when they state that

recovery is virtually impossible without it. The late Dr. William Hunt used to boast that he was the only known case in Philadelphia of recovery from perforation in typhoid fever, without an operation—a fact that would go to show how seldom such a recovery is. The mortality from operations undertaken for typhoid perforation will be very high, at all events, from the earlier ones. The patient who has a perforation is usually in a somewhat advanced stage of the disease, with a weak heart, and is likely to be extremely debilitated. The operation is one that requires not only skill, but exceptional skill. Everyone, perhaps, may not possess the requisite skill, and certainly the mortality that has heretofore existed does not compare with the results related by Dr. Price.

There is probably nobody in Philadelphia who does more operative work in abdominal affections than does Dr. Joseph Price, and for him to be able to report only three cases of operations after perforations shows that there must occur a large number of perforations in patients who are permitted to die without being given the chance of operation. For that reason the responsibility of operation is one that devolves largely upon the physician.

Dr. Price has raised the question of the effect of the anesthetic on the patient. While in some cases ether does bring up the pulse, at least for a time, it cannnot be maintained that patients do not suffer shock from operations. Almost all operations that are accompanied by anesthesia are productive of greater or less shock, and the truth probably is that prolonged anesthesia does diminish the probability of recovery.

Dr. Alfred Stengel agreed with the statement that intestinal perforations are of different sorts. In his experience they have very frequently been minute, or accompanied by a localized peritonitis, and rather gradual in their pathologic development, if not in their clinical manifestations.

While anyone who understands the conditions presented will not for a moment doubt the advisability of modern surgery in dealing with infestinal perforation, it must be realized that the operation in itself is one of the extremest difficulty. Very frequently the most successful, the most deft, the most experienced surgeon in this very particular line of surgery will fail to find the lesion or all of the lesion. In some cases there are several perforations, some of which may be found with the greatest difficulty post mortem, and this difficulty will arise in even greater measure to the operating surgeon. Complete resection of the bowel would, of course, obviate the necessity of looking for separate perforations in cases in which a limited part of the bowel is involved; but if trimming and stitching, and not resection are practised, it will be necessary to find each separate perforation. In some cases these are so covered by lymph and without marked extravasation of intestinal contents that some perforations are liable to escape notice. In Dr. Stengel's experience in post-mortem work, the intestinal lesions in cases of large hemorrhage, cases of rapidly fatal hemorrhage, have almost without exception been in the colon near its junction with the ileum, or in the ileum just above the valve. In these cases there are very large ulcers as a rule, and in a very considerable proportion this is the only seat of disease, ulcers being absent or very few in the ileum or lower part of the colon. It would be excessively difficult to manage such a case; and there may be some doubt that operation is indicated in these cases. Intestinal perforation in the course of typhoid fever is not a condition for medical treatment. There have been cases of recovery even with the formation of fistulæ, but these cases are so exceptional that they have practically no weight whatever in the discussion. When intestinal perforation has occurred the case may be considered out of the physician's hands. The question then is whether the surgeon wishes to undertake the operation. The physician's only responsibility in these cases lies in his calling in a surgeon; if the surgeon refuses the operation, the responsibility is upon him.

Dr. G. G. Davis related the case of a man who was brought into the hospital some two or three years ago, a foreigner, unable to speak English, totally delirious, with hurried, rapid, breathing, very weak pulse and a temperature of about 104°. The abdomen was very much distended and there was dulness in the right iliac fossa. There was absolutely no history to be had. The patient was evidently extremely sick and apparently at the point of death. It was thought that possibly there might be some trouble with the appendix. Operation revealed a distended cecum, while the appendix was found to be somewhat inflamed, but evidently not enough so to account for the general condition. Further investigation showed the small intestine adjacent to the cecum to be agglutinated in a plastic purulent peritonitis. At this point the patient exhibited symptoms of collapse, so that some of the adhesions were broken up, the surrounding area packed with gauze and drained and most of the wound left open. The patient died, and post-mortem examination revealed two perforating typhoid fever ulcers in the neighborhood of the ileo-cecal valve. The case serves to illustrate the difficulties attending operations of this kind. To have persisted in a search for the perforations and to have closed them would have caused death promptly upon the operating table.

Dr. J. C. Wilson said that the great majority of cases of perforating ulcer of the bowel in enteric fever perish promptly from general peritonitis and collapse. The exceptions to

this rule are extremely rare. In some few cases of perforation immediate general infection of the peritoneum is prevented by adhesions of the wall of the gut to adjacent viscera. Local peritonitis and abscess-formation, with its attendant dangers to life, result. There are also cases, but their number is few, in which all the signs of peritonitis develop and recovery takes place without subsequent trouble of any kind. It was a case of this sort, seen seven or eight years ago, that led to the communication referred to by Dr. Price. The patient was a girl about nine years of age who, about the twentieth day of the attack, suddenly developed symptoms of peritonitis. Dr. Keen was asked at once to see the patient with the view of operating. The necessary preparations were made, but after some hours, slight improvement having taken place, the operation was deferred until the following day. It was then found that the pain and tympanites had diminished and considerable improvement in the general condition had occurred. The patient made a good recovery without operation. She was treated by opium in full doses. Dr. J. Ewing Mears has suggested that the surgeon has three procedures at his command in the cases of intestinal perforation during enteric fever. First, he may find the lesion, trim out the involved portion of the gut and stitch the perforation; second, he can resect the compromised portion of the gut; and third, in default of being able to satisfactorily carry out either of these plans he may simply make an artificial anus, treat the peritoneum according to the indications in individual cases and wait. Of course these procedures are desperate, but the case is desperate and in ninety-nine out of every hundred accidents of this kind without surgery death is certain. The condition should be approached just as would be a fulminant case of appendicitis. The lesions are in the great majority of instances massed in the neighborhood of the ileo-cecal valve. Perforations rarely occur more than eighteen or twenty inches above that point in the bowel.

In some instances abdominal tenderness, excessive tympanites, tremor and hemorrhage from the bowels precede perforation. These symptoms must always be looked upon as danger-signals. Under such circumstances the possibility that prompt surgical intervention may become necessary is to be considered and it is a good practice for the physician in charge of the case to at once divide the responsibility with a surgeon, who can see the patient at intervals in consultation. The greater number of perforations, however, develop suddenly. When the symptoms of perforation show themselves the case practically passes out of the hands of the physician into those of the surgeon. The condition is so desperate that the surgeon often hesitates to perform an operation attended with so little hope of success. Speaking from the standpoint of the physician, Dr. Wilson contended that under such circumstances the physician is justified no longer in merely requesting, but he should at once demand that the surgeon shall lend his aid in the effort to avert impending death.

Dr. Joseph Price expressed regret that there is not a specialist in intestinal surgery in the world specially equipped and always prepared for emergencies. With the aid of such an operator more cases could no doubt be saved. Thus, in a case of typhoid under observation for two weeks, and running a uniform course, the physician, if a keen clinician, anticipating perforation, will suggest the association of a surgeon as soon as symptoms pointing to that condition arise. If there be not time to associate a surgeon the physician should use a pocket-knife, pulling out the gut and sticking a darning-needle in; then cleansing and draining with gauze. The result will be better than with no operation; but whatever the method used, it must be rapid and simple. It will never do to waterlog patients with ether. In none of the cases recorded did the operation last longer than one hour.

Dr. Price related that he had a great many times opened the abdomen for general suppuration in which he did not even seek the fistula, employing only irrigation and the open treatment. A large proportion of the patients recovered, although many were in collapse. Secondary operations in other cases have given evidence of the fact that ulceration had existed in the primary operation.

Dr. Price referred to a case of multiple perforation seen years ago, in which the patient's condition was so desperate on the table that the ether was withdrawn. The state of the bowel for some 12 inches was bad, but the defect was repaired as well as it could be, as the condition would not justify such extensive resection, and the ileum at a healthy point 20 inches from the ileo-cecal valve was connected with the colon. The woman recovered, and is in good health yet.

Referring to the transportation of these patients, Dr. Price said that his results have always been best in private practice. In a series of over two hundred abdominal sections in alleys and courts, with the nurse sleeping on an ironing-board or three chairs (until he could afford to have a cot taken around), with a cesspool four feet off, and everything calculated to be detrimental, only one case was lost, but such work required an enormous expenditure of vital force. Besides, nurses do not, as a rule, care to go into alleys or courts and do that sort of work, no matter how well they are paid. In a long series of cases the patient's home is to be preferred, if provided with the bare necessities of life, a couple of basins, a tea-kettle and water; but on account of the great amount of time

consumed in making visits at long distances, it is more convenient to have one's patients concentrated in one place.

Dr. Price insisted that operation in cases of typhoid perforation, to be successful, must be prompt; delay is fatal. Allusion was made to the case of a boy who had fallen from a hatchway and developed abscesses in three or more mesenteric glands. It was not thought that the fall was more than a coincidence, not a cause, that the boy was ill, that the typhoid suppuration in the mesenteric glands had been overlooked. Recovery followed section and open treatment. Dr. Price added that he had only recorded those cases that he was satisfied were typhoid. He had also operated on some of stercoral fistula that he is satisfied were typhoid perforations.

PROFOUND TOXIC EFFECTS FROM THE DRINKING OF LARGE AMOUNTS OF STRONG COFFEE.

J. T. RUGH, M.D.

[Read October 28, 1896.]

On July 10, 1896, E. M. F., a traveling salesman, came to this city, arriving about seven P.M., and with great difficulty walked to his hotel, about one square from the depot. He then complained of great nervousness and involuntary contractions in the legs and arms on attempting to move these parts. On arriving at the hotel, he went to bed, feeling that if he could rest for a while his condition of nervous excitement would pass away. However, he continued to grow worse, and about 9.30 o'clock sent for me. After carefully questioning him, I elicited the following history:

He is thirty years of age, married and has one child. He has always enjoyed good health, though he has not been very strong. He is a graduate in medicine, although he has never engaged in practice. The family history is negative, except for the occurrence of epilepsy on the maternal side. During his youth, the patient had numerous attacks of *petit mal*, but never any distinct epileptic seizures. He has never had an attack similar to the present one, and this dates back three weeks, when he started on a tour to New York to place several large orders for his firm. During the course of his work, he was up until one or two o'clock in the morning, and after retiring would get but three or four hours of sleep. Upon waking, he would order a pot of strong black French-drip coffee sent to his room and would drink this before eating his breakfast, which was very light and simple in character. In the course of the day, he drank ten or twelve large cups of this kind of coffee and ate but little food. If he went to a saloon with a customer, he would order wine or stimulant for him, but coffee for himself, and by this free use of coffee, he was able to fight off the fatigue which naturally attended this mode of life. This course was kept up for three weeks preceding his coming to this city. He drank some liquor, but not enough to produce any appreciable effect, and he was never intoxicated at any time in his life—a fact that is very important in the differential diagnosis, for a number of his symptoms were those of beginning *mania a potu*.

His pulse was ninety-six and full, but weak; his respirations shallow and numbering twenty-four to the minute. The pupils were normal, the tongue slightly coated, the bowels regular, the skin moist but not flushed, and his expression was agitated with the fear of some impending danger. His muscles were in such a state of tension that upon the slightest movement of arms or legs, clonic spasms occurred, though none was present when he lay perfectly relaxed, which, however, his exceedingly nervous condition would not allow him to do. If he tried to sleep, he would be seized with hallucinations just before losing consciousness, imagining that disasters were about to overtake him and seeing all kinds and shapes of images and objects. Then he would start up with fright and find himself in the

greatest nervous excitement. When he stood up, he could close his eyes or look at the ceiling without wavering. His knee-jerks were slightly exaggerated, but sensation was perfect.

The diagnosis of coffee-intoxication was based upon the history of excessive coffee-drinking for three weeks, the absence of liquor-drinking in quantities sufficient to produce constitutional effects, the nervous symptoms (spasms of muscles, hallucinations and extreme excitability), and the absence of an attack simulating *petit mal* in any way.

The indications for treatment were to clean out the bowels, to steady the heart by a stimulant, and to give an hypnotic that would quiet without increasing the mental symptoms already present. For the first, I gave calomel, one-sixth grain every half-hour for eight doses, followed by a saline. For the heart, I gave caffein citrate, one grain every three hours, knowing that this would at the same time partly offset the withdrawal of the coffee from his system. Trional (ten grains every two hours for three doses) acted admirably as an hypnotic. I did not give morphin, which might have had a quicker effect, because of its tendency, in many cases, to produce mental disturbances without thoroughly relaxing or overcoming the nervous tension of the patient. Three hours after this treatment was instituted, the patient was resting more easily and asked for a glass of milk, which he took with a relish. He then went to sleep and awoke the next morning, after five or six hours of sleep, very much refreshed. I interdicted the use of coffee, gave a tonic of arsenic, strychnin and quinin, and directed the man to spend several weeks at the seashore, as his former condition of *petit mal* was very liable to become one of true epilepsy unless he raised his system to the best state of health and maintained it there. He went to the shore and two weeks later reported himself as very much improved and feeling better than he had for months.

I have seen two persons who would be mildly intoxicated by drinking a large cup of strong coffee, but have never seen any one affected as this patient was, nor have I been able to find reports of any similar case in the literature of the subject.

DISCUSSION.

Dr. J. B. Roberts related a similar case seen ten years ago in a man under treatment for pneumonia. He was convalescing nicely, when suddenly he became very excitable. Dr. Roberts was sent for at night and found the man evidently intoxicated with something, but could not find out with what. The patient had not been taking any alcoholic stimulant of any account. Subsequently, on further inquiry, it was learned that some enterprising druggist had sent the man a sample bottle of bromo-caffein and that he had taken large amounts of it. The diagnosis was then clear. The symptoms had been distinctly those of caffein-poisoning.

Dr. J. Madison Taylor said that cases of peculiar susceptibility to drugs often have to do with a variety of causes, among them alterations in conditions of personality. He referred to a case of bromism in which the man became quite maniacal. The mania was of the sudden and violent order and it had been taken into consideration by those who first saw him that he was taking occasionally, for a very simple cause, bromids, which he increased on his own responsibility, with the result of inducing the profound effects referred to.

Dr. M. Price said that he has observed many cases of nervous excitability, inability to sleep and indigestion and other symptoms attributable to improper food in which investigation showed that coffee was the only substance to which responsibility could be attached; and on avoidance of the stimulant the symptoms disappeared.

THE PERFECT SURGICAL NEEDLE; WITH REMARKS ON COMMON DEFECTS IN NEEDLES.

JOHN B. ROBERTS, M. D.

[Read October 28, 1896.]

Little attention has been given to the proper construction of the surgical needle, though it is an instrument of great importance. A perfect surgical needle should be adapted for use by the surgeon's fingers without the interposition of any other instrument; its point should emerge exactly where the operator wishes; it should quickly and easily carry the suture through the skin or other tissue, and should be serviceable for sutures of silk, catgut, tendon, silkworm-gut, or wire.

The first proposition condemns as imperfect all needles that require a needle-holder. It always surprises me to see an operator encumber his fingers with a needle-holder in suturing ordinary cutaneous wounds. The explanation is probably to be found in the unsatisfactory needles often employed. The fingers are always better than any other needle-holder, unless the stitch is to be introduced at the bottom of a cavity, where the fingers cannot reach the wound. In cleft-palate operations and in vaginal surgery, needle-holders are necessary. Under nearly every other circumstance it is better to introduce the needle with the fingers.

Accuracy in directing the point of a needle, after it is buried in the tissues, and in bringing it out at the desired spot, is best attained by using a straight needle. It is difficult to ascertain the exact position of the point of a curved needle when it is once out of sight. This is attested by the frequency with which operators have been stuck by the point of a curved needle making its exit at an unexpected place. This uncertainty is lessened, but not entirely obviated, by the use of needle-holders or needle-forceps, which prevent the needle turning after its point is buried. A straight needle, guided by the fingers, is the proper means of overcoming the difficulty.

In order to fulfil the third condition, the needle must be sharp, have an eye large enough to be readily threaded with catgut, and make an opening in the tough skin sufficiently large to allow the head of the needle, with the two thicknesses of catgut, to pass readily. If catgut can be used in the needle, any other suture will go through its eye.

Within recent years various forms of needles have been offered by instrument-makers, but all that I have seen are inferior to the glover's needles which I have always employed. Some so-called surgeon's needles require so much force to drive them through the skin that a needle-holder or needle-forceps must be used both to insert and withdraw them. Some have such small eyes that they cannot be threaded with catgut; others cut such a small opening in the skin that the double thickness of the suture at the eye makes it almost impossible to drag the needle through.

Non-chromicised catgut, when softened with water, is probably the most bulky suturing material that we use. Surgical needles should therefore have eyes which can be satisfactorily threaded with this suture. They will then answer well for any other.

The perfect surgical needle which I show is only a carefully made and slightly modified glover's needle—that is to say, the needle that has for many years been found most satisfactory to those who are continually sewing animal skins in the manufacture of gloves and leather articles. The requirements are practically those pertaining to suturing ordinary cutaneous wounds. I doubt if any argument would induce a workman who stitches leather gloves or furs by hand to exchange his straight needle for the needle-forceps and curved, or otherwise defective needles often seen in operating rooms.

A needle has a point, a shaft, and a head. In the last is the eye. The perfect needle shown has a three-sided point, made like a trocar, and is very sharp. The sides of the slender trocar, as it may be called, must be exactly alike, so that the point will not resemble a bayonet, which has one of the sides wider than the other two. Some one, ignorant of the requisites of a surgical needle, has introduced to instrument-makers a bayonet-pointed needle. It is, in my opinion, inferior to the glover's needle.

The trocar-like point should occupy above one-third of the needle's length. The greatest diameter of the point should be near its middle. Although the end of the point should be very sharp, its three edges should not be keen, lest they cut the fingers when the operator pulls the needle through the skin.

The three-sided point gradually fades into the shaft, which must be cylindrical, not flat, and have a diameter a little *less* than that of the point where it is largest. The shaft then gradually tapers down towards the head, which must have a little *less* diameter than the thickest part of the shaft. The eye must be large, and oval or rectangular; not circular. The head, at the sides of the eye, must not be caused to bulge outwards by the drill or punch which makes the eye. Behind the eye the head must be grooved on both sides; in the groove lies the thread, which consequently presents no shoulder to catch as the needle passes through the skin. There is a groove in front of the eye on each side to render cleansing easier and to keep it free from dried blood and dirt.

The reasons for the characteristics of this needle will be apparent if one thinks for a moment of the work that a needle is intended to perform. It must make a hole in skin, a tough, fibrous tissue. The skin is very different from the woven materials sewed by the seamstress or tailor by means of a round or cambric needle. The instrument used by the surgeon, when he wishes to puncture a subcutaneous cyst by a small opening, is a trocar. A needle should have a similar point, which should be thrust through the skin with a sudden push, very much as a trocar is used. Some operators erroneously use a needle with the slow movement used in putting a pin into the end of a roller bandage.

In the second place, the opening made by the introduction of the needle must be enlarged, so that the shaft of the needle and the eye containing the thread may be drawn through it easily. This point is attained by having the point increase in diameter like a pyramidal wedge. When sufficient diameter has been given to effect this object the needle tapers down. The eye, with the threaded suture, therefore requires less or very little more space in going through the skin than the thickest part of the point, and glides through without catching or requiring force on the part of the operator.

To get the best service out of a needle the operator should occasionally sharpen its point on an oil-stone; and always select a needle large enough for the work. If the skin is thin and soft, as in the eye-lids, a small needle may be used; but when the incision to be closed is in thick skin, a comparatively large

needle is required to penetrate the tissues readily and easily.

A convenient method of preventing the needle becoming unthreaded, is to tie the short and long ends of the thread together at the eye by a half-knot. This is quickly done when the needle is threaded for use, and the half-knot is readily pulled out, if it be necessary to thread the same needle again after the suture has been used up. This tie serves to bury the thread in the grooves behind the eye. If a proper relation between the size of the needle and that of the thread exist, the knot will not catch as it is drawn through the skin.

In contrast with the perfect needle, I show a series of twelve needles, all of unsatisfactory shape, most of which are extensively used at the present time. Several are curved and hence difficult to direct, whether bent only at the end or throughout their entire length. One has a beveled point like a hypodermic needle, is cylindrical except at the very ends, and is wider at the eye than anywhere else. Nothing could be less well adapted to the purpose of a needle for cutaneous wounds.

Another—the Hagedorn needle—has a slender rod-like shaft, with a point sharpened like that of a knife. It is sure to go through the skin with difficulty, because it must catch where the suture makes a shoulder or ledge at the eye; and is very likely to cut the surgeon's fingers when he attempts to pull it through the skin. I believe this needle was originally advocated because it cuts a slit at right angles to the wound and the stitch, which lies in the end of the slit, tends to draw its sides together. Other needles were said to be less desirable because they had a tendency to make a wound with its long diameter parallel to the wound to be sutured. The tension of the stitch would, it was assumed, tend to draw this puncture open, and afford an entrance for infection. This reasoning is merely theoretic and of no value. The thread, as a rule, nearly or quite fills up the puncture, and if the needle-wounds are exposed to infective germs, the thread itself probably acts as a route by which they enter the tissues. Metallic sutures are less liable to act in this way.

One of the other needles that I dislike is cylindrical and of greater diameter at the eye than at any other part. Its unavailability is apparent. It may not have been originally intended for surgeons' use, but I bought it at an instrument-maker's. The bayonet-pointed needle I have already condemned, though it is much better than the others. Another unsatisfactory form has a flat shaft and a point somewhat like the head of a spear. Its fault lies in the fact that its widest diameter is not in the same plane as that of the thickest part of the threaded needle, which is at the place where the suture occupies the eye of the needle.

Finally, I show three needles which have a trocar-like point similar to that which I call the perfect needle. They are, however, exceedingly bad, because the points do not make a puncture big enough to allow the shaft and eye of the needle to traverse the skin with ease. No adequate provision is made in them to dilate the wound or to have the after-coming head of less size than the shaft of the needle.

DISCUSSION.

Dr. Joseph Price contended that the resistance encountered in passing needles does not take place at the point mentioned by Dr. Roberts. Erichsen, in his *Military Surgery*, attributes death to tight sutures and suture-tracts. Charles Hunter called attention to the fact that a suture should always fill up the needle-track. With the needle proposed it does not. This needle offers great resistance in its huge belly, between the point and shank of the needle, more so than any needle in use. This will cause more difficulty in penetrating tissues than any other needle in the group presented, more suture-tract abscesses, more liability to clot along the track of the needle, more suppuration, pain and sepsis. In Dr. Price's opinion surgical needles should be fine, straight and sharp, with a perfect point, and without belly and cutting edge. Keith's is the ideal needle, which can be placed nearly everywhere. Tetanus and sepsis have a number of times followed the use of the Baker Brown or Peaslee handled needles—that old bayonet-needle, with a handle, commonly used for perineal work.

Dr. G. G. Davis said that for certain deep-lying tissues the Hagedorn needle is pre-eminently suitable and not bad elsewhere. The glover's needle is an absolutely useless needle. While workers in leather may know what they want, it is not good enough for surgeons, because, as sold, it is not polished. It is too much to expect nurses to sharpen three-cornered needles or needles of any kind. Instrument-makers themselves not infrequently fail to furnish well-sharpened needles.

Dr. Geo. E. Shoemaker said that it is not uncommon to see a man with a towel, trying to push a needle through the skin and uttering imprecations. This is due largely to the oxidization of the needle in boiling. For some time it has been Dr. Shoemaker's practice to sterilize needles by holding the eye in an alcohol flame before the operation; then, throwing them into alcohol until needed. Treated in this way, needles need not be boiled. They always keep bright and pass through the tissues readily; and their use is unattended with complications.

EIGHT PRIMARY MOVEMENTS OF THE NORMAL SPINE AS A BASIS FOR GYMNASTICS IN THE TREATMENT OF SCOLIOSIS AND ALLIED CONDITIONS.—A PRELIMINARY REPORT.

J. T. RUGH, A.B., M.D.

[Read November 11, 1896.]

The successful treatment of scoliosis and allied conditions of the spine is attended with many difficulties, among which may be mentioned the chronicity of the condition, the marked deformity when the case is (usually) seen for the first time, the rigidity of the spine and its related parts, the great length of time required by the treatment, the work necessary on the part of the patient, and the multiplicity of exercises necessary to prevent the monotony that is sure to follow the continual performance of the few heretofore prescribed.* The age, habits, temperament, etc., of the patient are also important factors in the prognosis and treatment, but far less essentially so than those first mentioned.

The combined method of treatment, viz:—by exercises and apparatus—is the one most favored and most frequently employed by orthopedic surgeons at the present day. It has for its aim the prevention of further increase of the deformity and, at the same time, the development of the atrophied and weakened muscles and the increasing of the flexibility of the spine. By a systematic course of treatment is secured the muscular development necessary to maintain correction when once secured and to the proper performance of the more powerful positional exercises that are designed to overcome the deformity, and increase the mobility and flexibility, and, at the same time, carry to a higher degree the muscular development begun by the first and simpler movements. In a given case requiring exercises, it is easy to say to the physician, "give exercises and restore the function of the spine and you effect a cure." He may readily grasp the principle of treatment, but as these exercises must be given through a long period of time (varying from one to three or four years), the problem of multiplying them becomes of the utmost importance. This is especially true of the physician who has not made an extensive study of the deformities of the spine or who seldom has to do with such cases. For guidance he turns to any authority upon the subject and finds thirty or forty or more exercises given as being indicated in scoliosis and similar conditions, and with these he begins the treatment. This supply is soon exhausted and he is at a loss how to proceed further. He finds that he must either cease treating the case, or repeat the same exercises over and over, and then the patient tires because of the monotony of their use. The continued repetition has grown irksome and the improvement is so slow that interest in the work is soon lost and further treatment is suspended. One rarely sees a patient who will perform the same movements three times a day for a year or so; but with properly selected gymnastics, and many of them, persistence is easily maintained and a cure of the deformity is the reward of both patient and physician.

The employment of these exercises

*Text-books usually direct from twenty to thirty exercises to obtain a cure.

has always been a matter of empiricism, one writer quoting those given by another, and perhaps adding a few of his own, but laying down no rational basis or system by which further treatment is governed. A physician may secure sufficient for a given case, if he be of fertile resources, or have a number of text-books, but reproduction of these becomes more or less a matter of memory when he has another similar case to deal with, which makes the process burdensome as well as very irrational. Tubby, of London, whose work on *Deformities* is the most recent contribution to orthopedic surgery, divides scoliotic patients into two classes: "those in whom muscular weakness is pronounced," and "those in whom muscular development is fair;" and he gives about twelve or fifteen exercises for the treatment of each class; yet he furnishes no directions for further gymnastics. This is the same method that is adopted by all writers on the subject and the treatment of such deformities has, in this particular, remained at a standstill. Some statistician has collected over four thousand exercises without gymnastic apparatus, which are applicable to the treatment of scoliosis, but to remember one-eighth of this number would not only burden the memory, but also would be unnecessary when the different forms can be so easily systematized and rendered available for use.

After I had formulated the plan herein embodied and had almost completed the preparation of this report, I found that Noble Smith (Curvatures of the Spine, 2d Ed., p. 14), had recorded very concisely the movements of the normal spine as follows: "The spinal column can be moved anteriorly, posteriorly, or laterally; or these movements may be combined in circumduction. The whole spine can also be rotated upon its own axis;" and with this declaration he dismisses the subject and directs exercises in the treatment of spinal curvatures empirically, enumerating one form after another to be used in certain cases and conditions. He apparently did not recognize the fact that the cure of these cases demanded the restoration of the normal function and consequently the employment of these natural movements for that purpose. With the following eight movements as a basis, a rational system of exercises can be formed, and for purpose of demonstration, I will again enumerate them.

1. Bending forward.
2. Bending backward.
3. Bending to the right.
4. Bending to the left.
5. Combining these movements in circumduction to the right.
6. Combining these movements in circumduction to the left.
7. Rotation of the spine upon its vertical axis to the right.
8. Rotation of the spine upon its vertical axis to the left.

This description of the movements of the normal spine is in accord with that of anatomists, who describe the normal motions of joints as flexion, extension, adduction, abduction, circumduction, and rotation, and while these terms do not cover all the motions of which a joint is or may be capable, yet all others are, perforce, modifications of these primary forms, and it is this conception of the spinal capabilities that is of so great assistance in multiplying and directing new and proper gymnastics for the treatment of these most obstinate conditions. Even as direction in space is referred to the four cardinal points of the compass, so all spinal movements may be classified under these eight simple forms, and all belong to or are modifications of bending, twisting, or circumduction.

In all cases of scoliosis or allied spinal deformities, there is more or less rigidity of the spine present, due not to an inflammatory or other pathologic condition of the bony or muscular structures, but to the muscular debility, inactivity and atrophy, and to the long-continued assumption of the mal-posture. With this rigidity, there is also limitation of motion in one or more directions. The exact amount of such rigidity and limited motion can be ascertained by having the patient perform these primary movements and noting the degree of flexibility of the spine in each. The guide to the gymnastic treatment is thus

obtained, and exercises that will increase the motion in the desired direction can be prescribed. Since all movements, then, are limited to eight cardinal forms, it is evident that variety must be had by change of position, and in many cases, if not in all, this is the most important part of the treatment, as, by this means, total correction of the deformity in some cases and partial correction in all cases, may be obtained. While, therefore, certain attitudes are indicated in certain cases, they are directly contra-indicated in others; hence the importance of this part of the treatment.

The relations that the arms and legs bear to the spine are of course most important as altering the contour of that part. For instance, raise or extend an arm and note the curving of the spine toward that side. The same obtains in the case of the legs, because of the tilting of the pelvis upon changing the relative length of the legs by lifting or moving one of them in any direction. A lumbar curve may be obliterated by this action, or a dorsal curve by the proper placing of the arms; and by the combination of these two, the "key-note" positions, or those in which the greatest amount of correction obtains, are assumed, and a better result is obtained in the treatment.

What positions, then, may be used for the purpose of multiplying exercises? All those that to a greater or less extent obliterate the existing deformity, such as lying prone or supine upon the floor or table, sitting on the floor, on a chair or on a stool, with the legs in various positions, or one hip raised, standing with feet together, feet separated, one advanced or retracted, standing on one foot, using the rings, trapeze, or bars, kneeling on both knees or on one knee, squatting, and so on, without number. But in all these positions, the spine can be moved only in the eight primary ways.

In this short paper I do not discuss the mechanism of these motions and positions and their importance as altering the deformity and restoring the function of the spine, as these will be considered at length in a more or less elaborate study of this subject, to be published later. In bringing this subject before you, I do not wish to be understood as originating a new form of treatment for scoliosis and other similar spinal conditions; but the object may be briefly stated as:

1. To remove the employment of exercises from the realms of empiricism, where it has so long held sway.

2. To place it upon a scientific and rational basis.

3. To make this basis so simple that every physician can readily grasp the underlying principles.

4. To place within every physician's reach a complete system with which to rationally meet the requirements of each and every case.

5. To make it possible for the physician to personally direct the forms of exercises when it may be deemed expedient to refer the patient to a masseur.

DISCUSSION.

Dr. De Forest Willard said that Dr. Rugh has done a valuable service in systematizing and classifying the movements for the rectification of scoliosis, for it is essential that there shall be some definite plan. While no two cases of scoliosis can be treated exactly alike, as the curves will differ in each individual, yet there should be a definite and specific object in view. It is difficult for the ordinary practitioner, unless he has given the subject great attention, to understand exactly what is to be accomplished, and more difficult to accomplish the result after deciding upon the treatment necessary in that particular case. The curves differ so greatly in each that they require especially careful study to decide in the first place the method to be employed in the rectification. It is simply impossible for anyone to decide unless he examines the bared back of the individual and examines it most critically and carefully in the various positions of sitting, standing, walking, bending, etc. Unfortunately, many of these cases occur in young girls at the age

when they are particularly modest in the exposure of their backs, and yet it is absolutely necessary that these movements shall be studied and regulated by the physician, to accomplish results that will in that individual best overcome these curves; it is therefore essential that they be tested with the posterior aspect of the trunk uncovered.

The restoration of normal movement and the relief of rigidity are the important portions of the treatment. It is the rigidity which is so difficult to overcome, particularly when rotation has occurred. In the general profession the distinction is not made with sufficient accuracy in regard to the cases of slight functional lateral curvature and those in which rotation has actually taken place. All who have experience in this disease know that the profession is too prone, upon the detection of a slight functional lateral curvature, to suppose that the application of an apparatus will assist or cure the individual. There can be no greater fallacy. No case of functional lateral curvature was ever benefited in the slightest degree by an apparatus. Very many have been injured and very many rendered hopeless by the rotation that has occurred after the apparatus has been applied. The only method by which a slight functional scoliosis can be corrected is the restoration, in the first place, of the muscular balance of the two halves of the body and the restoration of the curves to their normal position. This can only be accomplished by muscular action, by the tonus of that muscular action. This is to be brought about by special gymnastic movements, not by general calisthenic movements, although these come in sometimes with good effect in producing increased tone of the general muscular system; but the special movements set forth by Dr. Rugh are upon the plan that every one must adopt if he expects to cure these cases and protect them from relapse. Each surgeon will probably adopt variations of these movements, but their general purpose and plan will be the same.

Posterior and lateral rigidity are most marked and these are the two conditions which must be overcome, so that the flexibility of the spine in these directions shall be made more in accord with the normal. When the normal condition is restored, the individual is cured for the time being, but not permanently. It is exceedingly important that these cases should receive for a long time careful oversight, and that they be watched until they have attained full growth. The girl should not be allowed to become careless in regard to her walking and standing, or in regard to her muscular exercises, until she is eighteen or twenty years of age at least. The muscular tone must be maintained by appropriate exercises, even after the case is considered cured.

In cases with rotation, the question of apparatus may be relegated to the individual ideas of the person treating them; there are cases in which apparatus does possibly assist in giving a better form to the individual. That, unaided by special muscular exercises, it ever benefits a rotated spine is not likely. It is simply a support, to give a better conformation in adults exteriorly, or to hold what has been gained by gymnastics, and it is perfectly proper in old and fixed cases. But in cases in which cure is expected no dependence should be placed upon it, without the added effect of regulated movements. With regard to light and heavy gymnastic exercises there is, of course, the same difference of opinion as there is in regard to heavy and light gymnastics in general. A succession of light movements systematically repeated is, however, far preferable to any system of heavy gymnastics, unless an individual has the time and can give herself absolutely and entirely to this work; but, unfortunately, the majority of cases afflicted with this condition are not able to do this.

Dr. H. Augustus Wilson said that gymnastic exercises are capable of producing and have produced more serious damage and more permanent injury than do properly applied remedial gymnastics. Especially is this noticeable in spinal curvatures in general, because it is now generally understood by the laity that remedial gymnastics give the best results, the more quickly obtained and with less danger of relapse than almost any procedure that has ever been advocated for the treatment of rotary-lateral curvatures. It, however, is very much like the painter, who, when asked how he was able to obtain such wonderful effects, replied that he mixed his paints with brains, and this applies with equal force to gymnastic exercises. The serious danger to be produced by gymnastics has been caused by lay administration, absence of system, absence of any scientific, rational basis and the necessity for resort to first one text-book and then another to obtain a series of exercises that are empirical, and without apparent basis. In gymnasiums and so-called physical institutes, such a plan is carried out, and when the various systems or series of exercises are exhausted there is nothing further to be done. The directors of these institutions are practitioners of medicine, but without medical education and training and therefore without a license to practise medicine. When cases come under the direction of these untrained (at least medically untrained, although possibly well trained as far as gymnastics for the normal individual are concerned) persons, take, for instance, cases of lateral curvature, they put them through a system of gymnastics whether properly applied or not and turn them out at the end of a year "strong, well and cured" patients (?). It has been Dr. Wilson's misfortune to have seen at least twelve cases in which conditions other than scoliosis were present (several were cases of Pott's disease, one a case of comparatively

recent fracture of the lumbar vertebræ) and they were all put through the most severe forms of exercise, pulling heavy weights, etc. As an explanation of the plan pursued, reference was made by the gymnasium teachers to some six or eight text-books from which exercises had been taken and advice for curvature of the spine followed. The result has been that, as in the case of the painter referred to, the colors were not mixed with brains and the subject of gymnastics was brought thereby into contempt.

Dr. Rugh has done a great service in placing before the medical profession a definite basis for treatment in cases that are susceptible to it. He has done one thing more by showing how to give the patients themselves an incentive to further work and exercise in the right direction, always under the supervision of a physician. One of the most difficult things in the world to accomplish in physical training is to give the patient an incentive to act in co-operation with the physician in the matter of these exercises. To tell the patient to go to a machine, for instance, and pull chest-weights a certain number of times, will, after a time produce definite results, not always beneficial. If the patient knows that the effect to be produced on himself is a certain one, he will watch constantly to accomplish this, and this co-operation of the patient will far more efficiently and satisfactorily aid in effecting a good result. In this way it is possible for the patient to be instructed by the physician at the right time that eight primary movements of the normal spine constitute the goal to be sought in the correction of these abnormal spines and they will work toward this end and it will enable them to go on far more rapidly, intelligently and satisfactorily.

One of the conclusions in Dr. Rugh's paper is so important as to deserve special attention, that is, "to make this basis so simple that every physician can readily grasp the underlying principles," and, with the principle in hand, the physician will be able to modify the monotony of forms of exercise to produce important results.

The word "cure" has never been clearly defined. There often seems to be a mistaken notion that cure means entire restoration to normal condition and normal function; such can hardly be the case, or else the cures constantly reported are not cures. Cure must mean simply arrest of destructive processes and restoration as far as possible to a normal condition. If such is the case gymnastics certainly offer the best means of obtaining a cure in rotary lateral curvature, because while they may not and cannot in ultimate bone-deformity of spine produce a restoration to the normal condition, they will produce the nearest approach possible to the re-establishment of normal function and general appearance. One of the most serious mistakes that it if possible to make is to attempt to combine forms of gymnastic exercises, as has been suggested, with directions that the spine should be held straight during the interim by braces and appliances. It is granted that during this interval of the remedial gymnastics false postures may be assumed, but the gain in muscular coordination and improved mobility of the spine is largely lost when restraint of movement is induced by apparatus.

The spine is simply a series of joints, the whole being for convenience considered to be in the form of one joint. Any joint of the body will be described as to its functions just as the spine is. If a joint becomes ankylosed the restoration to normal function will be obtained by following the normal movements, and the same course must be followed with regard to the spine. The more this subject is studied the more it will lead physicians to discourage the disastrous damage of the more or leas hap-hazard methods of applying gymnastic treatment at the present time. Such powerful therapeutic measures as remedial gymnastics should be administered only by competent physicians familiar with the pathologic, diagnostic, and physiologic aspects of the subject.

Dr. J. K. Young said that orthopedic surgeons have for many years been aiming at the action of the muscles, the complicated combined action of muscles which are exercised. There are at least eight different curves produced in lateral curvature. Very few of these are simple or primary, but they are usually combined together in some form, and the most common form of all is right dorsal primary scoliosis. Surgeons who treat these cases usually combine exercises, so as to fit the particular form of curvature. In this particular form of curvature the object aimed at is to develop the muscles that are weak, to lengthen short muscles and to shorten long muscles, to correct malpositions of the pelvis and the different changes that occur in association with this curve. In right dorsal lateral scoliosis there are on the concave (left) side a number of muscles that are shortened because the bones are too close together. On the other (right) side there are a lot of muscles that are stretched. One limb is shorter than the other. This has usually been spoken of as a short limb, but Dr. Young was inclined to think that in the functional forms it is a long limb. The limb can be shortened on one side and the pelvis may be straightened and fixed firmly in that position. If the muscles of the extremities are well developed, then there is a foundation to build a straight spine on. The curvature of the spine can then be corrected. By placing the patient with the low shoulder high, with the left arm elevated, certain exercises can be given in this position which will develop muscles on the concave side, and also shorten muscles on convex side. In addition to these movements, others are also of aid, and massage must be given regularly every

day, from five to fifteen minutes, by the surgeon himself, or a trained masseuse or an attendant of some kind. The exercises are to be given not empirically, but with a clear understanding that the object of the treatment is to shorten long muscles and lengthen short muscles, to restore the pelvis to its normal position, and to erect upon it a firm, healthy spine. After this has been accomplished, relapses must be prevented. The treatment is intended first to develop weaker muscles, to prevent rotation, and to avert relapse.

Dr. J. P. Mann said that over one thousand different forms of exercises have been collected for the treatment of scoliosis. That is one of the reasons why the treatment does not produce better results, and in giving a basis upon which these deformities can be healed scientifically, Dr. Rugh has done the right thing. There are some other things, however, in the treatment of scoliosis to be borne in mind. In the first place, all cases of scoliosis arise from some local lesion or defect, from some constitutional condition, or, it may be, from a combination of both. In a case of rachitic scoliosis, or in a case in which there is a vertebra that has not developed or several vertebræ—and cases have been reported in which there has been entire absence of two or three of the vertebræ and ribs and clavicle—in such cases as these, of course, the treatment must be entirely different from that in cases of scoliosis due to some local condition. Take some local condition: A child has infantile palsy; the leg is shortened thereby, and from the malposition that the child takes, a scoliosis gradually sets in. Again, some set of muscles around the spine will be affected by paralysis. This is another local condition that may cause scoliosis. These instances sufficiently indicate that the trouble may come from constitutional and also from local causes. When these cases present themselves, the spine is either flexible, and we can straighten the curve more or less, or the spine is fixed and we cannot straighten the curve at all. In some of these spines, from changes in the bones, from pressure and from absorption of the disc of cartillage between the vertebræ, bony ankylosis takes place, making a rigid spine. It is useless in cases of this kind to attempt by any form of either exercise or apparatus, to cure the deformity. It is not useless, however, to try to limit the further progress of the primary curvature or any secondary curvatures that may occur. In the treatment of the flexible cases the plan that Dr. Rugh has outlined certainly gives a good basis to start from. No man can pretend to remember one thousand different forms of exercise, because they do not proceed from any scientific basis. It is merely a feat of memory to treat cases with them, so that with a definite form of exercise, in these cases in which there is flexion of the spine, the surgeon may hope to bring about arrest of the condition in the worst cases and to cure in some of the cases that are not so bad.

As to mechanical treatment: If the patient can be kept under the surgeon's care—that is the flexible cases, when the exercises can be directed and attended to by the patient—no mechanical form of treatment is wise. But, when the patient is not accessible to the physician, or when there is no person to properly direct such exercises, a suitable brace will help to arrest the progress of the trouble.

Dr. Bertha Lewis said that before proceeding with the eight primary movements of the spine described, it as necessary to adopt a method of treatment that will promote the efficiency of the circulatory and respiratory functions, and which, at the same time, does not overlook the fact that the endeavor to improve the muscles may succeed at the expense of both heart and lungs. To secure the best results, the physician and not the gymnast is the best person to conduct all remedial exercises, provided he or she has been fitted for this work and possesses an interest and patience that are necessary to carry it to successful ends.

The work of Mr. Bernard Roth, of London, is a striking illustration of what can be done on these lines. The "keynote" of his treatment consists first in developmental exercises until the tone of all the voluntary muscles of trunk and limbs has been raised to a working value, by *consecutive* daily treatments, for a period of not less than three months. These are to be followed by prescribed exercises that may be carried out at home. Not only does Mr. Roth require the greatest accuracy and precision in each exercise, but every movement that the patient takes, and her carriage in walking or sitting at rest are cared for. Of equal importance as a remedial measure is the dress of the patient, not merely while exercising, but also that habitually worn should be loose enough for perfect freedom of motion.

With the exception of the work done in the Swedish Institutes, where the Ling system is used, Dr. Lewis has seen no work that produced such successful results as Mr. Roth obtained, and which are largely the result of his careful personal treatment of each case. The orthopedist is no more justified in sending a case of rotary lateral curvature to a gymnasium than in sending a patient to a druggist with a diagnosis and allow the latter to select the remedy.

Dr. Mathilda K. Wallin, of New York City, said that the special movements referred to were not at all new to her, because being brought up in Sweden and a graduate of the Royal Gymnastic Institute, she was already familiar with them. She had, in fact, been taught no others as the primary movements of the trunk, the flexion and extension of the back, the rotation, the circumduction

and the side flexion, although the same terms were not used. All the exercises were, according to Ling's system and teachings, divided into the so-called "fundamental positions," and from those the movements or positions as "derivative"—the idea is the same, the working basis, so to speak, the same; only the terms differ.

It has been said that there were too many exercises, but the point is to have the right ones, or to be able to select the right and beneficial ones in each case; to discard the poor ones and use only those that are best. Many of the exercises are used without proper discrimination and are worthless. Physicians ought to be *thoroughly* educated in these matters before attempting to use the exercises in the treatment of scoliosis. They ought to know all the different exercises, not only those of one system or set of gymnastics found in text-books, but they should know them all; their action, their mechanism, when to apply them and how to apply them in all the different cases.

As has been said and emphasized, a patient should never be sent to a gynasium for the treatment of scoliosis or for any treatment. First of all, the gymnasium people do not know anything about the pathologic conditions, as a rule; they do not know how to treat a patient, who may often grow worse instead of better; and secondly, it should be understood that a gymnasium is for well people who wish to keep their health and strength and for those who need general development.

There is no question that gymnastics constitute the only measure or the best measure in the treatment of lateral curvatures of the spine, and the gymnastics should not be too *light*, either, after the general development and muscular tone are first obtained, nor too heavy or too strong.

Of all the different systems, there is none that compares with the so-called "self-correcting method," combined with the Ling system. If the two are combined the results are greater or more successful than by any other system; but it takes an immense amount of time; the patients have to work steadily, and some give up, in fact, the whole day to it. The system is carried on in Norway, and is hardly known outside of that country. An apparatus similar to the suspension apparatus is used, each child having its bench for resting and a belt with handles to take hold of for support and fixation of the shoulders. The idea, in a few words, as it is impossible to describe it intelligently without demonstration of the movements, is that the patient, in a fixed and correct position, by the efforts of his own will and work—certain movements—shall strive and be able by this muscular effort to steadily extend, correct and straighten the spine and its curves. Wonderful results may be accomplished by this method, in combination with Ling's gymnastics, but the patients come and work almost the whole day, with proper rest between the exercises. Of course they are almost all children, and come for the purpose of being cured and can generally afford the time.

Dr. Benjamin Lee said that with regard to the general question of scoliosis or rotary lateral curvature, he has long been of the opinion that the primary curvature does not, as a rule, take place in the dorsal region, but in the lumbar region and he agreed heartily with the remarks as regards the importance of establishing a firm basis for the spine in the pelvis. It is necessary to correct the deformity of the pelvis in order to correct the deformity of the spine that rests upon it. It is therefore Dr. Lee's rule in the treatment of these cases, and especially of those seen early, to pay especial attention to the lumbar portion of the spinal column and to correct any deformities and irregular muscular contractions that exist in the lumbar region and in the upper femoral region. There are in almost every case of lateral curvature contractions more or less marked of the abductors and of the psoas muscles. Attention should be directed to these groups of muscles. As regards the personal corrective treatment alluded to, it is Dr. Lee's habit to instruct patients to make use several times a day when at home of certain postural treatment, which after a little time they are able to carry on quite intelligently themselves, and this proves an important addition to the treatment in the gymnasium.

As regards the cure of lateral curvature everyone knows that to attempt to cure advanced cases of lateral curvature of the spine is to undertake an impossibility, that is to say, to restore the patient to symmetry; but there are other things than symmetry to be considered in a patient suffering from lateral curvature. Very serious conditions of the lungs and of the heart are present as a result of advanced lateral curvature, and no case is so advanced that the patient may not be given reasonable hope of checking further deformity and of so increasing the chest-capacity as to relieve the heart and lungs from at least a portion of the disability under which they are suffering. The physician is, therefore, warranted in undertaking the treatment of cases of very serious deformity, when he cannot at all hope to restore the patient to symmetry, with the expectation of increasing the chest-capacity and in that way relieving the pressure on the heart and lungs.

Dr. Young said that there is a difference of opinion as to whether or not right lateral scoliosis is the most common form of spinal curvature. Abroad left lumbar scoliosis is said to be the most common and in this country right dorsal scoliosis, sixty-six per cent. of these occurring to about thirty-three per cent. of the left; the left lumbar then being still less frequent than the left dorsal. Dr.

Young's remarks were confined entirely to the treatment of the atonic or mild functional cases of scoliosis, following the distinction made by Dr. Willard, of cases of functional lateral curvature and those in which rotation has occurred. A picture was shown of a model with a curvature, who had been posing for two years, standing constantly for long periods upon the right limb and in this way developing a left primary scoliosis.

DRY HOT-AIR APPARATUS.

Dr. H. C. Wood presented to the Society Mr. Lewis A. Tallerman, who came from London with a letter of introduction from Dr. Lauder Brunton. Mr. Tallerman delivered a short address on the Tallerman-Sheffield Patent Local and Medical Dry Air Bath for the localized application of dry air heated to a high temperature.

The efficacy of the apparatus was tested on two patients from the Philadelphia Hospital: one a case of saturnine gout, the other one of lumbago. Dr. H. C. Wood said that Mr. Tallerman is the inventor of a new method of treating chronic rheumatism, rheumatoid arthritis, sprains, both acute and chronic, and a large number of general inflammations and rheumatic affections. There have been published in the journals, especially in the *Lancet*, a number of very remarkable results said to have been obtained by the use of the apparatus of Mr. Tallerman, who will briefly state the principles of its construction and show its immediate mechanism, and then it is proposed to put the apparatus to the test on two patients whose treatment will require from forty to sixty minutes, so that the results of the treatment can be seen.

Mr. Lewis Tallerman said that the therapeutic effects produced by the Tallerman-Sheffield Localized Hot Air Apparatus and Treatment are several. Among them are an enormous increase in the circulation, profuse perspiration of the whole of the body, relaxation of the part, dilatation of the vessels and an increase in the body-temperature, which it has hitherto been regarded as impossible to obtain from local applications of heat. The temperature is usually raised under treatment from 1½° to 4°F. and returns to the normal in from fifteen to twenty minutes after the operation. In rheumatic affections, rheumatoid arthritis, gout, lumbago, sciatica, sprain and kindred disorders, pain is relieved or entirely removed at the first operation and the patient usually expresses the great relief he feels and his ability to move his body and limbs with greater freedom. The progress of the disease is arrested and a curative process is set up at the first operation and it is a remarkable circumstance connected with this relief that the limbs and body outside the apparatus participate in the benefit, and that the benefit is not limited to the part placed in the apparatus and subjected to direct treatment. The Tallerman-Sheffield process is absolutely painless and soothing throughout, and, if permitted, patients under treatment would frequently fall asleep. In very chronic cases and when the disease has advanced to that stage at which the joints are held rigidly fixed by very firm, fibrous adhesions, it is found better and more expeditious to treat the limb for a short time so as to prepare it for more active interference and then to break the adhesions down under an anesthetic. By this method there will result little inflammatory reaction, and if the limb is placed in the apparatus and treated immediately the patient wakes from the anesthetic, the heat will have a remarkable anodyne effect and most of the pain will subside in a short time.

This method has now been adopted in several large London hospitals, including St. Bartholomew's. Mr. Alfred Willett and Mr. Walsham, and other medical men had witnessed the effects of the treatment in a preliminary trial and this was followed by two of these apparatus being sent to that hospital and they were tested in a series of selected cases over a period of two months. At the expiration of that time Mr. Willett delivered a clinical lecture* at that hospital and pointed out the great value of the treatment in certain cases. The lecturer pointed out that in so many surgical cases would the apparatus be serviceable that he hoped that it would not be long before one was available at any time at the hospital and also a nurse trained in its use. He also called the attention to certain medical cases which by analogy he thought might be treated on these lines with great benefit.

This was followed by an invitation from the North-West London Hospital, at the suggestion of Mr. Mayo Collier, where a series of test-cases were treated over a period of two months. The results in these cases were published in the *Lancet* for January, 1895. Among these were cases of rheumatoid arthritis, which had been under treatment at the hospital for two years and had been treated prior to that at the University College Hospital. One patient, a woman of 64 years of age, was, according to the hospital notes, quite unable to dress or even to feed herself. Dr. Knowsley Sibley, the physician in charge of the case, kept her under observation for about

* Published in the *Clinical Journal* for May, 1894.

two years to ascertain the permanency of the benefit and then exhibited her at the Medical Society of London, with other cases. She had been kept under treatment for two months, but after the eighth operation she was able to return to her work as a seamstress and has been continually at work since, now over two years. For case-notes and reports upon the case, the *Lancet* for June 6 to August 29, 1896, may be consulted. In one case a temperature of from 250° to 260° was experimentally applied for over two hours, and that without the skin becoming unduly sensitive. There is a very great increase in the circulation, and morbid matters and morbid products are thrown out of the system, leaving a healthy condition of both joints and tissue.

Dr. Frederick A. Packard demonstrated a case of saturnine gout that had been extremely obstinate to all forms of treatment in so far as his articular symptoms were concerned and had at the time suffered from severe pain in one of the big toes, which was constant in spite of all that could be done. On learning of the Tallerman-Sheffield method of treatment, Dr. Packard thought that the case would be a good one for demonstration because the man was suffering so much pain in the big toe that it could not be touched or flexed, and also because he had tender nodes on the hands, so that the effects on peripheral processes far from the seat of treatment could be seen.

Dr. H. C. Wood, in answer to the question, "Is not moisture produced by the great heat, and liability of scalding present?" said that the only moisture about the apparatus is that from the limb and this is said gotten rid of by opening valves and letting out the air from the cylinder. He presented a case of lumbago in which it was supposed the apparatus would afford marked relief.

After the demonstration Dr. Wood pointed out that the man with lumbago, who had moaned and groaned when touched and had been in the hospital a week without much benefit was, after the treatment, able to get up.

The case of saturnine gout had been under competent medical care for some weeks, without much gain. The man had not moved his toes for three months, but after the treatment he was able to move the toes freely. In fifteen minutes after the treatment was begun he could move his toes, while the pulse went up 20 beats and the temperature rose 1.4°; the other man's temperature went up about a degree in forty minutes and his pulse increased nine beats and gained very distinctly in fulness. The temperature within the apparatus was 260° and the pelvis was kept in it for forty minutes. The patient with saturnine gout stood a temperature of 260° for three minutes; then it had to be reduced to 255°, which he stood for some time. Most of the time the temperature was 248°.

Mr. Tallerman said that he would prefer these patients to have two or three applications to make assurance doubly sure. He would not feel satisfied with a single application that it would give the best and permanent results. It is his custom in such cases to repeat the treatment a few times.

Dr. A. G. Reed inquired if pain in the fingers was relieved by the application to the foot.

Mr. Tallerman replied that it was.

Dr. Max J. Stern asked how the cases would be on the morrow.

Mr. Tallerman replied that the muscles would probably be stronger, and the present good effects continued. If this were a patient being treated by a physician, he would not be satisfied to let him go. The physician would prefer to treat him once or twice more to make sure.

Dr. W. S. Stewart asked what precautions were taken to avoid bad after-effects from the rise in temperature.

Mr. Tallerman said that the patient was briskly rubbed down with a rough towel, and he had never known any bad effects to ensue.

In cases of rheumatoid arthritis, when patients are strong enough to allow of it, he treats them first for four or five times a week and afterwards three times a week. He stated that there was a smaller apparatus for the hand.

Dr. H. C. Wood said that the immediate results achieved by the apparatus were remarkable and that it is stated on the highest authority in England that permanent results are reached; so that the apparatus seems to be an important addition to our therapeutics in a class of cases that have been exceedingly anxious and worrying to all.

REPORT OF WORK WITH ROENTGEN RAYS AT THE POLYCLINIC HOSPITAL.

MAX J. STERN, M.D.

[Read November 11, 1896.]

It is my purpose to demonstrate by means of photographic plates and prints some of the results attained during the last six or eight months in the Roentgen-ray laboratory of the Philadelphia Polyclinic. I am impelled to do this, rather than present a paper dealing with the physical side of the subject, in the belief that to the surgeon and the general practitioner the greatest amount of interest still attaches in this new field to the tangible results that can be pictorially reproduced and with certainty demonstrated. While the present paper will of course deal with generalities only, it is my hope that I may, from time to time, present such accurate detailed knowledge as I shall have gained from my almost constant labor in this new diagnostic field.

I shall briefly recount the technic pursued in obtaining the pictures which I shall present. The various procedures are carried out in a large, airy, darkened room. The light is excluded not because of any effect it may have on the photographic plate or that the chemic changes induced on it by the X-rays are in any way interfered with, but because the amount and color of the light in the tubes can be the better observed, and the retina, accustomed to the surrounding darkness, is much more sensitive to an appreciation of the value of an image on the fluorescent screen. The deeper the green light in a tube and the greater its coincident intensity the more X-rays are being given off. When the light approaches a yellow color the visual or fluoroscopic picture is usually better, but the radiographic effect poorer.

The electric energy is derived from a chlorid accumulator storage-battery of the series known as "Type 5 E." This has been by far the most satisfactory of all the various kinds of batteries I have tried. The Ruhmkorff coil is one with an eight-inch spark and the regulation spring-vibrator. I have not employed the brake-wheel. The Crookes' tubes I am at present using are the new three-electrode tubes devised by Prof. Stein of the Chicago University and made for me by Messrs. Queen & Co. They have proved the most satisfactory so far employed. Their distinguishing feature is that when the vacuum in the tube becomes high, as it almost always does after a long exposure, it can be sufficiently reduced to make the tube again available by a change in the polarity of the electrodes and a reversal in the direction of the electric current in the tube. The stand for the tube and all wires employed are insulated as perfectly as possible, to reduce the electric leakage to a minimum.

The patient or subject to be exposed to the rays is placed in a position most favorable for either visual observation or exposure to a plate, as the case may be. This is only too often a matter of much study and frequent experimentation. Thus, if it is desired to obtain an antero-posterior radiograph of the knee-joint for instance, the natural conclusion would be to lay the subject on his back. place a plate under the posterior aspect of the joint, and the

tube above the patella and make the necessary exposure. True, a very acceptable picture of the joint can be obtained in this way, yet when compared with one taken in the reverse position, i.e., with the sensitized side of the plate in contact with the anterior surface of the limb and the tube excited against the popliteal aspect, it will be found that the exposure can be reduced almost, if not quite, one-half, and the resulting picture be infinitely sharper and hence more beautiful in its contrasts. What has been said of the knee-joint is practically applicable to almost every other portion of the human body. It can be readily understood then, that, at times, when long exposures in apparently awkward positions are necessary, much ingenuity must be exercised to place the subject in such position that the enforced quietude shall not become to irksome. For this purpose a bed, tables, racks, bed-rests, various-shaped chairs, splints, pillows, bandages and assistants must be at hand, or the operator will meet with obstacles that appear insurmountable.

The sensitive plate I now invariably use is X-ray plate specially prepared by Mr. John Carbutt of this city. It is by far the most rapid and most generally satisfactory of all the plates I have employed, and I have tried all the more rapid ones, and a number of the slower of those most commonly employed by American photographers. The ordinary plate that I have found to approach most nearly the specially prepared X-ray plate is the Carbutt B. 16. I am firmly convinced that the best plate has not yet been produced and our plate-makers must devote their attention to the evolvement of one that shall respond much more rapidly to the X-rays than do those at present employed. My belief is that some fluorescent material may be embodied in the emulsion that by its absorption of, or agitation by, the rays will greatly enhance the activity of the chemic change in the silver-salts employed. The plates as furnished at present by Mr. Carbutt are packed separately, first in black needle-paper, then in a black envelop and finally in an outer sealed one of yellow post-office paper. This packing renders them light-tight and with ordinary care will prevent fogging.

The remaining necessity for obtaining a successful picture is the fluoroscope. This is a box shaped like a truncated pyramid. The inner surface of the base is coated with a fluorescent material, in this country almost invariably of calcium tungstate; abroad, apparently always barium-platinum cyanid. The upper end is open and lined with a furry material. When the observer's eyes are placed within this aperture the box becomes light-tight. At present I shall only consider the fluoroscope in its relation to obtaining a successful radiograph. When a fluorescing screen or fluoroscope is directed to an excited Crookes' tube, its base or sensitized surface becomes luminous. The fluorescence can be compared to the impression received in looking from a darkened room through a sheet of ground glass into light beyond. An object interposed between the tube and the screen, will, if sufficiently impervious to the rays, give a shadow-picture. The more impervious the substance to the X-rays, or the greater the mass of the same substance, the darker will be the resulting shadow. For this reason our pictures show sharp values in substances of different densities or more delicate ones in tissues of the same character but of greatly varying thickness.

We measure the activity of our tube, or the amount of X-rays that is being given off, by the brilliancy of the fluorescence of the calcium-salt and the sharpness of definition and detail shown in the shadow-picture of an interposed body, which for convenience is usually the hand or arm of the operator. In general terms, the more active the tube (by this I mean the volume of X-rays that is being given off) the shorter the exposure. This time is fixed by the operator and is entirely dependent on his judgment, which is formed from the relative value of the picture presented on the screen. In other words we use a fluoroscope as a sensitometer.

Having briefly detailed the necessary apparatus I shall review the succeeding steps followed in obtaining a picture.

Most subjects seem to be timid about submitting to the essential exposure, probably because the method has not been sufficiently employed to make the procedure familiar to the general community; but unless they are exceedingly stupid, particularly refractory or intoxicated, little or no difficulty is experienced in removing all dread of evil consequences. In children, however, the task of preventing movement, and, not infrequently, violent struggling is often a most difficult one, for one cannot reason their fear away, and the noise of the vibrator, the electric sparking and the general gloom of the darkened room, frequently arouse their worst fears, more particularly if they have been the subject of some painful injury. A little diplomacy, a coin or confection will, however, often produce astonishing results. After the exposure has begun and proceeded for some little time, the subject soon quiets down, a feeling of languor follows, and not infrequently, sleep. I shall have occasion again to refer to this effect.

The part to be portrayed always contains a bony portion, which lies nearer to one surface than another. I am, of course, considering only the human frame. Now this is an axiom: the closer the contact between the sensitized plate and bone, the shorter the exposure and the less the distortion and the sharper the resulting picture. Whether injury to bone is sought to be portrayed, or a foreign body to be located, the law remains unchanged. In the latter instance the nearest bony prominence that can with accuracy be demonstrated must serve as the landmark from which measurements must be directed and calculations made. Failure to grasp this point has led to frequent and trying disappointments. It is but an illustration of how essential is an anatomic knowledge even in successful skiagraphy. The best picture will always be obtained with the plate in contact with that surface of the body which is nearest the underlying bone that is sought to be most sharply portrayed in the skiagraph.

The position of the tube in relation to the part is largely a matter of experience. The tendency of all operators at the beginning of their career, is, I presume, to have the tube in too close contact with the subject. This has several unfortunate results. For one thing, the picture will always be distorted, and lack sharpness of definition. Should a light be gradually moved from an object, a point will be reached where the shadow will give an image, fairly reproducing the body of the original, and showing sharp lines. This point would be the proper one at which to place the tube. To prevent burning, however, is the more important reason why the Crookes' tube should be at a sufficient distance from the patient. We know that a portion of the body may, with safety, be exposed to the diffuse rays of the sun, yet, should some of these rays be collected by means of even a mild convex glass and directed to the same part, the result would be a burn, the degree and severity of which will depend upon the duration of the exposure and the amount of surface involved. I believe the X-rays produce such an effect when the tube is held too close to the body. There is an area about the center of the field of activity of a Crookes' tube which is very much more powerful than any toward the periphery.

All those cases of so-called sloughing reported in the newspapers are, I believe, nothing more or less than burns of the first and second degree, produced by long exposures with the tube placed in improper relation with the part to be skiagraphed.

The necessary exposure will be shorter (1) as the spark-gap is larger, up to a certain point, probably about ten or twelve inches; (2) the more nearly the vacuum of the tube is at that point of exhaustion at which the coil employed will give off the greatest amount of X-rays; (3) the nearer the tube to the proper focal point; (4) the less the density and volume of the tissue in the part to be portrayed; (5) the more sensitive the plate.

The exposures of the body vary from that of a finger or toe, which requires the least time, to that of a hip-joint, which requires most. With the coil I employ I have obtained very beautiful

pictures of the hand and forearm in two minutes, but it requires at least an hour to get even a fair representation of the hip-joint of a well-developed adult.

The development of the plate and its subsequent treatment admit of wide latitude and are matters I shall not at present discuss. The printing, however, must be considered briefly. In the first place, almost all highly glazed papers will give better results with X-ray plates than will mat surface papers. More or less of the detail will be lost in the latter; yet for exhibition purposes, they are better, as the reflection of light from their surface is less, and they can be more readily seen by a large assemblage. Any of the good albumin or collodion papers of glossy surface will be found best for X-ray work. Much of the beauty of a finished picture is due to the treatment of the plate, after it is placed in the sun to print, and those desiring to acquire a knowledge of this branch of X-ray work should first serve an apprenticeship in a photographic gallery, as I did. Frequently certain parts must be printed out and others restrained, depending entirely upon the part of the picture it is desired to emphasize.

There is another subject that I desire to discuss briefly, the incidental effects of an exposure. In regard to the former I may say that in an ordinary exposure of a limb in a person not of an excitable temperament little or no change can be detected in the vascular or nervous system. If, however, the tube be directed over the thorax of even one who has frequently been exposed and has neither dread nor other cause to suffer reflex excitation of the cardiac centers, a decided acceleration and change in the character of the heart-beat can be detected. The rise in an assistant of mine amounted to as much as fifteen beats in the minute. The change in the character of the beat can also be detected in sphygmographic tracings, as was shown in the case of a patient suffering from a large thoracic aneurism who had been exposed a number of times and in whom no cause for excitement existed.

DISCUSSION.

Dr. Arthur W. Goodspeed called attention to one or two possible improvements in the form of a tube that has not reached the market yet, a tube that was given him two or three weeks ago and which he has been using very successfully for most of the work he has done this fall. It is very much smaller than the ordinary tube in common use, and is especially designed for maintaining its exhaustion permanent. The trouble has been that the tubes would become too highly exhausted with use and special devices have been necessary to obviate this difficulty. One device is to have a third electrode. The first Crookes' tube of focus type used by Dr. Goodspeed (April 6, 1896,) was obtained from the physical cabinet of the University of Pennsylvania, where it had been for fifteen or twenty years. It has two electrodes besides a platinum disc in the middle; so that there is nothing new in having an extra electrode. The fact, however, that the exhaustion can be varied by expert manipulation of these electrodes is a matter of great importance.

The new tube referred to is coated with some fluorescent substance which seems to occlude on its surface a certain amount of air which is set free by the action taking place in the tube. If the action is too great the tendency to raise the exhaustion is counteracted by the setting free of the gas spoken of; so that to a certain degree the exhaustion of the tube may be maintained constant. If such a tube be worked too roughly, that is at too high an electromotor force, the exhaustion is likely to run down. This may be remedied by allowing the tube to rest a few minutes.

Prof. Hutchinson of Bowdoin College, it was believed, is the originator of this tube. Dr. Goodspeed's experience is not sufficient to give a professional opinion in regard to it yet, but he believes it will be a success.

In regard to the statement of the effect upon the skin Dr. Goodspeed's experience has not agreed with that of Dr. Stern. The difference may perhaps be either in the apparatus itself or in the method of using it. Dr. Goodspeed has not used the tube as close to the surface as seems to have been done in one case—never closer than six inches. Dr. Goodspeed's own hand has been in contact with X-rays for hours at a time since February of this year and he has never noticed any effect whatever upon his own flesh or upon any of the patients who have been sent to him by physicians. This effect upon the skin may point to some conclusion which may be evi-

dence in favor of one of the two or three theories that are being discussed now in the physical laboratories in regard to the ultimate nature of these X-rays. One of the theories is that the X-rays are of the nature of light, of very short wave-length. The other theory, which seems to have been gaining ground very rapidly of late, is that the X-rays are material particles which are shot off from the exterior surface of the tube by the bombardment of particles within the tube and that these pass through the fleshy material of the subject and produce their impress upon the plate. If glass particles are shot off from the outside of the tube and are impacted in the flesh why should not some physiologic change be produced? If this matter is followed up and record made of cases, with the degree of severity noticed, valuable evidence may perhaps be secured in favor of the material theory. Therefore those who have cases in their charge should make records of all peculiar conditions, as these records may be valuable in the future.

Mr. John Carbutt spoke only on the practical side of radiography. In the use of the plates, a great deal of success lies in the development. The plate on which most of the subjects shown were made is prepared with a fluorescent substance in the emulsion which causes it to absorb X-rays and prevents their passing through the plate. It was early observed in the use of the dry plate in X-ray work that on the ordinary plate the developed image showed almost as soon on the back as on the front. It was at once concluded that the X-rays had such penetrability that a greater part of the rays must be lost. This loss was attributable to the prolonged exposure in the early part of this year, some of the plates being exposed from one to one and one-half hours by Prof. Goodspeed. In the second or third week in February, Mr. Carbutt took at the Maternity Hospital, for Prof. Magie, of Princeton, a radiograph of a woman's hand, and with one of his most sensitive plates, with an exposure of twenty minutes, got merely a silhouet of the hand without showing bone-formation. He then employed the special or X-ray plate, giving the same exposure, and got full delineation of the bones of the hand. Dr. Goodspeed made an exposure of one the same day in twenty-five minutes and good negatives were secured from that time to the present. Mr. Carbutt said that a good deal of the best work had been done by Prof. Goodspeed, and good results have been obtained in from four to seven seconds. The average time for those who make radiographs is from two to three minutes for the hand; thicker parts of the body, the elbow or arm, require from ten to fifteen minutes.

In the use of the Crookes' tube, one has to study the color of the rays that are produced inside the tube. When it has been working for some time, the kathode rays will be greenish-yellow, and these are best for observation with the fluoroscope, but when the rays are of an apple-green color, they give the best photographic effect. The development of a plate made especially for this purpose should be continued from six to twenty minutes, because the film, being of double thickness, requires a longer time for the developer to penetrate. On developing such plates, rarely will any image be visible on the back even after ten minutes in the developer. Mr. Carbutt has placed two of them together and the upper one yielded a fine negative and the lower had a mere ghost of an image. In experimenting on the penetration of celluloid films, Mr. Carbutt has put eight films together and they have yielded six very good negatives, showing that glass is a greater preventative of the rays passing trough. He has under way now experiments with an entirely different sort of soluble haloid, with the hope that a silver haloid can be obtained which will be more sensitive to the X-rays.

Comparing the action of the rays on a plate to that of daylight through the lens, he found that the longer the plate is exposed to the rays, the more dense the result on the margin, which is just the reverse of what takes place with daylight; as, if an over-exposure in the camera takes place, only a thin flat image results, and the exposure may be continued so that a reversal of the image is secured; but with the X-rays, the longer the exposure, the greater the density, the silver haloid being so acted on where the rays have had full action as to give a thicker, heavier deposit so opaque that light cannot pass through it. A radiograph of a rat was exposed fifteen minutes, yet the margin of the background is so dense that it remains white when the image is fully printed, and that without intensification of the negative.

Dr. Max J. Stern said that in making skiagraphs of the thorax, he placed the tube at as great a distance as thirty inches. He has found that the greater this distance, the more effective the result and the better the picture. The bad result in the case of burn reported was brought about by having the tube very closely applied, but this was an exceptional case. In making skiagraphs in in cases of metatarsalgia, the plate was placed under the sole of the foot, as the erect posture is that in which most pain is felt in this affection. In reproducing hand or foot, it matters little whether the plate is above or below, because the distance to be traveled is so small.

Dr. Stern suggested the possibility of making a plate that could be used immediately in emergency cases when one desires to use a plate at once. He referred to a plate fashioned after the old ferrotype plate, and expressed the hope that in a short time a print will be available that can be used within two or three hours of the taking of the picture.

THE NEURON.

ALOYSIUS O. J. KELLY, M.D.

[Read November 25, 1896.]

Until within comparatively recent years the generally adopted conception of the nervous system was that its essential tissue was made up of two distinct morphologic elements, nerve-cells and nerve-fibers. The nerve-cells were supposed to originate, send, receive, and modify impulses. These were conveyed from one nerve-cell to another by the nerve-fibers, acting as conducting agents and serving to connect nerve-cell with nerve-cell. This transmission of impulses presupposed continuity of structure. We now know, however, that the nervous system is not made up of a number of nerve-cells maintained in continuity by nerve-fibers, but that it is composed of a number of distinct and independent neurologic units called *neurons*.

Each neuron originates as a unit, structurally independent of every other neuron, and as such it remains, despite its subsequent morphologic complexity. The essential parts of each neuron are the *nerve-cell* (in a restricted sense), the *axon* (axis-cylinder process), and the terminals of the axon,—the *end-tufts*. The nerve-cells are of various shapes and have received appellations in conformity therewith. The axon is a differentiated process of the cell-body. It may preserve its individuality and proceed as the axis-cylinder of a nerve-fiber, or it may immediately break up into numerous fine filaments. The axon always terminates in a free extremity,—*end-tufts* or *end-bushes*. Further, there proceed from the cell-body other processes known as *dendrons* or *dendrites;* and from the axon other processes known as *collaterals*. On the dendrites there are fine hair-like projections called *lateral buds* or *gemmulæ*, and at the branching of the dendrites thickenings known as *varicosities*. It is of a multitude of such neurons that the nervous system is made up.

Each neuron is always structurally unconnected with any other neuron. The relation that one bears to another is simply that of propinquity, or possibly contact. The function of the gemmulæ is to receive the nervous impulses from the end-tufts of the axon (for instance), and transmit them to the dendrites, whence they are conveyed to the cell-body proper. The impulse is further carried throughout the neuron by the axon, which thus serves as a cellifugal conducting apparatus; the dendrites transmitting impulses cellipitally. The impulse is delivered to a muscle-fiber, for instance, occasioning contraction, by the terminals of the axon, which in order that it may distribute the impulse over a large area divides into numerous fine filaments, the end-tufts. These latter, therefore, serve as *organs of emission* or *deliverance* for the impulse. The collaterals functionate as do the axons.

Without doubt the most important part of the neuron is the nerve-cell, with the dendrites, being the nutritional, trophic, receptive, and impulsive elements. The dendrites are parts of the cell-body, being split-up portions of its periphery. They resemble the cell-body in structure and in function. The nerve-cell has been aptly

termed the vital part of the neuron. The axons, collaterals, and end-tufts, outgrowths of the cell-body proper, are of secondary importance. They conduct and deliver impulses to neighboring regions. The processes of the neuron are, as it were, projections of the cell-body into the various motor and sensory regions of the body. Thus, without regard to its topographic situation, there is no one part of the nervous system that is not in direct association with every other part. There is no one part that functionates absolutely independently of every other part.

The theory of the motility of the neuron aptly explains certain hysterical, hypnotic, and other functional states (sleep), and may also serve to account for certain morbid manifestations, the nature of which we do not understand (tachycardia)*. Various so-called system-diseases of the nervous system are now known to be due to disease of neurons functionally allied,—a system of neurons. The fact that the more distal part of any axon (as the axis-cylinder of a nerve-fiber) is the least resistant to the morbific influence of various agencies, permits of our comprehending the occurrence of peripheral neuritis due to alcohol, arsenic, etc. We have also been able to discover the anatomic basis of certain mental diseases.

DISCUSSION.

Dr. Charles K. Mills said that the word "neuron" was first used about 1884 by Professor Wilder of Cornell University, who applied the term to describe the cerebrospinal axis, but afterwards discarded this usage for "neuraxis." From some remarks made a few months ago by Professor Wilder, he seems inclined to return to the use of "neuron" for cerebrospinal axis. Waldeyer, about 1891, suggested the word neuron in the sense used by Dr. Kelly. The term itself and the ideas that circle around it have a certain fascination; even the mere use of the term has done something to clarify our ideas with regard to the structure and functions of the nervous system. Dr. Mills was personally inclined to the views as to the terminology of the nerve-cell suggested by Schäfer, which have been adopted by Donaldson of the Chicago University. In teaching and writing Dr. Mills largely disregards the use of the word "neuron," but it is necessary to use it as a synonym, at least, as it has doubtless come to stay. A neuron after all is a nerve-cell. Accepting it in Waldeyer's sense, as used in describing the anatomic nervous unit, it is the nerve-cell and all of its processes, both dendrites and axis-cylinders. The word "neuron" is certainly sometimes convenient in description, and yet this convenience in its use may occasionally lead to trouble, particularly in the description of such a complicated nervous apparatus as the auditory, when it becomes necessary to speak of neurons of the first order, second order, third order, etc. In this way more or less confusion in teaching and writing may arise. Physiologic misapprehension may result from laying too much stress upon the neuron as an independent anatomic unit. The discovery that the neurons, or nerve-cells are independent anatomic units has let in more light upon nervous structure and action both physiologic and pathologic than anything that has been done in recent years. It must not be forgotten, however, that there are myriads of these nerve-cells in the nervous system, and that while they are independent anatomic units (and this point should be strongly emphasized) only in rare instances are they functionally independent. They are arranged together in special groups, and have many elaborate relations to each other, and therefore, in the physiologic sense little or no independence of action.

When it is recalled that only a few years ago the farthest shore to be reached in the knowledge of nervous structure was indicated by the five or six cortical layers of Meynert and Bevan Lewis, and that now in a single stratum of a single layer of the cortex some thirteen or fourteen independent systems of cells and fibers can be traced, one cannot but be impressed with the great value of the studies of the neuron or nerve-cell, made possible by new methods of staining. What such discoveries will lead to in the future no man can tell. Dr. Kelly has indicated in a few directions the results that have already been achieved, particularly in the pathology of chronic alcoholism and some of the forms of dementia.

Dr. F. X. Dercum said that to him terminology is of secondary importance. It is the substance of one's thoughts which is of the most significance. He held that the term neuron, if used at all, should be applied to

* The Medical and Surgical Reporter, October 24, 1896, p. 513.

the nervous integer and not to a mere part thereof. Schäfer's suggestion to call the nerve-fiber the neuron would lead to endless confusion.

The term neuron should be applied to the nerve-cell, including all of its processes, both the dendrites and the neuraxon. The word cell should be restricted to the cell-body. The dendrites are of course a part of the cell, just as the pseudopoda of the ameba are part of the cell. Still it appears better to speak of them separately as dendrites, just as we speak of the process that forms the nerve-fiber as the axon.

These facts have radically changed our conceptions of the nervous system. We no longer speak of cells and fibers, of trophic or other centers, but regard the nervous system as made up simply of cellular elements, as are all the other tissues.

Our misconceptions in the past have been largely due to our physical impressions. We often reason automatically without stopping to think whether our conceptions are legitimate. Thus when thinking of the nerve-cell, we habitually call to mind a stained hematoxylin or carmine preparation, or now, perhaps, we form a mental picture of the blackened spider-like appearance of the cell as stained by the Golgi method. These conceptions have led us to regard the nerve-cell as something absolutely fixed. The truth is, the nerve-cell is a minute mass of protoplasm with living extensions running from it in various directions, and this conception leads naturally to the idea that the nerve-cells possess some power of mobility. This is as interesting a suggestion as any of the others that are conveyed by these new facts. It is very probable that the nerve-cells have some real mobility, although Cajal suggests that this is indirect. He makes the neurogliar cells the active integers. It seems to be far more rational—far more in keeping with what we know of cell-life—to accept the view that the nerve-cells have independent movement.

There is one observation made by a German investigator, Wiedersheim, in one of the transparent enteromostraca, which proves absolutely that the nerve-cells in some of the lower forms really do move.

The possibilities that the movement of the nerve-cell opens up are exceedingly interesting.

Dr. J. K. Mitchell said that innumerable suggestions arise for possible applications of the theory of the neuron. The conceptions of interference with transmission of nerve-influence that can be deduced from the infinite complexity of the neuron—communications are much wider than in the old, ruder conception of the transmission by nerve-cells practically united into one group—suggests how very small disturbances of the neuron connections might make very great change in the amount and character of transmission of influence, and infinite possibilities in the subject of the regeneration and degeneration of so many minute and intricate fibers spreading in various directions.

Dr. A. O. J. Kelly said that the term neuron, being more or less new, has a certain fascination, and having been suggested to describe a structure, the individuality of which heretofore was not even suspected, it has justification in fact and will persist. It is doubtless its availability that has caused its almost universal adoption. In the paper read, the word has been used to describe the entire cell, including its prolongations, and not after the very confusing manner that Sch fer has seen fit to employ it. As knowledge increases, this terminology may be changed, but probably not materially. The terms that have been used are those at present more generally adopted. Certain of the physiologic and pathologic suggestions, to which allusion has been made, have opened up new thoughts, and it is just beginning to be understood how much the future may have to offer.

AMBULANT TREATMENT OF FRACTURES OF THE LEG.

EDWARD MARTIN, M.D.

[Read December 9, 1896]

DR. EDWARD MARTIN presented a communication entitled

"AMBULANT TREATMENT OF FRACTURES OF THE LEG,"

and demonstrated the method of applying the dressing, also exhibiting patients. The method of treatment consists in so applying the plaster dressing that a patient, instead of being confined to bed for five or six weeks, can get about and attend to any light occupation. The dressing had been applied to twelve patients in the last six or eight months, and it was hoped that all would be present, but only five were able to come. Dr. Martin has employed the dressing in upwards of thirty cases and the results have been good. There were two cases in which the results were not thoroughly satisfactory, but in these Dr. Martin did not apply the dressing personally. One was an instance of Pott's fracture. The deformity was not greater than is often noticed after treatment with the fracture-box, but there should have been none. The other was one of compound fracture, for which the patient was operated on. The wound healed kindly at first, but later it broke down and the fracture was found to be ununited. The bone was then cut down upon, and a portion of the tibial shaft, about two inches, resected. A few days after the operation, as the operation-wound was running a sterile course, Dr. Martin directed an ambulant dressing to be applied. The patient was up and walking five days after the operation, and he apparently did well, the bone uniting, although there was some deformity. Dr. Martin has never used the dressing for fractures of the bone above the knee, and with the exception of the last case all have been simple fractures.

The first patient exhibited was a boy, who was run over by a bicycle, and both bones of the leg were broken at the middle third. There was no difficulty in replacing the bones in good position, and an ambulant dressing was put on a few days after the injury. After the plaster was applied there was no pain, and the patient has been walking ever since—that is seven weeks ago. There seems to be less stiffening and after-trouble than is common when the fracture-box is employed. The boy has been very comfortable and has been saved a long confinement in bed. The first dressing applied broke, because of poor plaster, and a second one was put on about ten days later.

In the second patient, a boy, the first dressing was poor and a second had to be applied. The latter was worn for five weeks. The union is perfect and there is no disability.

Dr. Martin explained that he was not advocating this method of dressing particularly; nor was he trying to prove that it is better than any other dressing. He merely wanted to decide what its real merits are, and he was simply taking testimony.

The third case was one of fracture of both bones of the leg at the middle third, during a fight. The patient began walking the day after the application of the dressing. He has been coming to the Dispensary to be dressed, and has been saved several weeks in bed.

The fourth case had a Pott's fracture; the patient began walking the next day, but had some pain.

The last case was in an old woman, who fell down stairs some three or four months before, breaking the upper third of the tibia and fibula. There was very little deformity, and the dressing was applied promptly. She had less use of the dressing than any patient that Dr. Martin has thus far treated. She could not walk without a crutch or chair. She did not go home for three weeks. She did not complain of pain, but did not seem able to manage the large, heavy dressing. The day following the application of the dressing, the second after her accident, Dr. Martin took her by the hand and led her up and down the ward, but the walking was not rapid or easy. Perhaps the best thing the dressing did for this patient was to shorten her stay in bed and allow her to go home and walk about one or two weeks sooner than she otherwise would have done.

She presented absolutely no deformity; nor did any of the cases.

The cases presented were not selected by any means, and represent very clearly what can be expected from the dressing. The first proposition to treat fractures by the ambulant method was made by Bardeleben, in 1894, and following him, a number of German surgeons tried it. Bardeleben reported upwards of 111 cases. Some of his colleagues reported a number of others, so that the total number was 200. Since that time Dr. Martin has had upwards of thirty cases of his own.

The Germans apply the ambulant dressing not only for fractures of the leg, but also for simple and compound fractures of the thigh. Some students of Dr. Martin's in Germany at the time of Bardeleben's first statement reported that the results in fracture of the thigh were unsatisfactory, marked deformity often resulting.

The principle on which this dressing is applied is that of fixation and shifting of support. Given a fracture of one or both bones of the leg, as soon as the weight is thrown on the foot there is, of course, a tendency to overlapping of the bones. If the weight of the body could be carried from above the fracture to the ground by external supports, as by means of a plaster case passing from the knee-joint to below the sole of the foot, the tendency to overlapping would be at once corrected. If, at the same time, the weight of the body could be not only kept from causing overriding of the fragments, but could also fix the fragments in good position, there seems to be no good reason why the patient should not use his leg. This is accomplished by the so-called ambulant splint; the plaster is so applied that the weight of the body is carried from the tibial tuberosities just below the knee, to the reinforced portion of the dressing lying below the sole of the foot, pressure upon the malleoli being avoided by abundant cotton padding. This dressing fixes both the joints contiguous to the fracture. It is, perhaps, open to a serious objection, as it entirely prevents the use of massage. It is inapplicable to certain cases, *e.g.*, when there is great swelling, when the shape of the leg is such that the weight cannot be carried from the tibial tuberosities to the rigid dressing that envelops the leg (this is particularly the case in very fat women with small bones). In most instances of simple fracture of the leg, when reduction is possible, either with or without ether, and when the swelling is not marked, the dressing is the most satisfactory that can be used.

The dressing should be applied as soon after the fracture as possible. It requires abundant padding with some elastic material. It must be so arranged that the plaster does not press into the malleoli, and the plaster must be neatly fitted below the knee; it should be worn from four to six weeks. On the day following the application of the dressing, the patient is gotten up, with the help of an assistant or a friend, who takes him under the arm and assists him in walking up and down the ward. The Germans use an apparatus very much like the walking device provided for children, the patient standing inside a sort of cage which is on wheels. In a day or so walking is possible with two crutches; in another day, with one crutch, and in a week the patient can usually get along with a cane or without any support. It is scarcely rational to apply this dressing to a hod-carrier and expect him to be able to continue his employment at once; but a man can attend to ordinary business pursuits.

Another advantage of the dressing is that less atrophy follows than after the use of the ordinary fracture-box or plaster dressing. Dr. Martin held that the fracture-box is a poor dressing. Even if the patient is lying in bed the plaster is best. The atrophy is a manifestation of a reflex neurosis, as has been proved by experimental research. The great advantage inherent in the plaster dressing is that it enables the patient to be up and about and that anyone who realizes how irksome it is to stay in bed when it is not absolutely necessary may perhaps save these patients four or six weeks of lying on their backs.

As to the direct method of applying the dressing: In the first place the plaster must be fresh and that is often a very great trouble in plaster bandages. They are better made from a coarser mesh of gauze than that used for the ordinary surgical bandage. A lot of cotton batting should be provided and the fracture must be reduced. A mistake is often made in not etherizing more frequently than is done in the treatment of fractures. The first essential is thorough reduction. When there is swelling and the patient is sensitive there is a great temptation to trust that reduction is accomplished without making this perfectly apparent. In perhaps the majority of cases in applying this plaster bandage Dr. Martin gives ether. He always does this when it is not absolutely certain that the bones are not in proper position. The leg is then enveloped in a flannel roller, as is customary in the application of all plaster dressings; the cotton is wrapped about and then the plaster is applied.

For the purpose of flanging out the lower part of the plaster and giving a little more strength, Dr. Martin has had made some slightly flexible steel rods, such as are used in orthopedic work for bracing shoes. They are flexible, so that they can be molded to the desired shape. In putting on the dressing it is essential that the toes should point a little upward. If they drag they are liable to strike things. The heel should be a little lower than the toes. A child of eight or ten years of age requires a flannel bandage two inches wide. The foot is covered in, no reverses being employed. In putting on the cot-

ton batting it is well to tear it off the required width and it then rolls out very much like a bandage. The malleoli are thickly padded, so that the plaster may be prevented from falling in and pressing at these points. At the knee-joint where the plaster should bear pressure the batting is thinner. If the surgeon intends to take the plaster off himself it is well to partly cut it and for that purpose a lead strip is needed. The sole of the foot is padded with a mass of cotton at least three inches thick. The plaster is now ready for application. The first two turns take in the padding on the sole and keep it in the middle line. In applying this bandage a little tighter pressure is employed than with an ordinary plaster bandage, as, because of the thick padding, the parts will stand it.

When the patient is placed under the influence of ether, he must be kept under until the dressing is hard. The bandage runs up to a little above the knee. Having closed in the cotton the external metallic splints are applied. These are molded to the part and covered in with more plaster. The patient is kept perfectly quiet and the toe is held up until the plaster hardens. In another twenty-four hours the patient is allowed to put the foot to the ground. While the plaster is still moist a knife is run along the line of lead strip, leaving several bridges, which should be marked by pencil before cutting. When the time comes to remove the plaster the bridge is simply cut through by a penknife and the dressing will spring off. Even plaster dressings that have not had this preliminary cutting can be best removed with the penknife. The legs as they come out of this plaster show some wasting; the ankle is stiff, but no more stiff than it is in the fracture-box.

DISCUSSION.

Dr. Ernest Laplace stated that the method demonstrated impressed him as being a very good one, especially for such cases as lying in bed would be injurious to. It is to be preferred to other methods, because it allows the patient to go about and to preserve his ordinary health, instead of being weakened by being kept in bed. It should be applied in every case in which the patient would otherwise suffer from being kept in bed. All patients when kept in bed will be lowered in vitality, especially children. When children are kept going about, the nutritive processes are maintained and the formation of callus is more rapid. The treatment has the further advantage that if the leg is well extended while the splint is being applied there is no danger at any time afterward that contraction from undue irritation of the muscles in the neighborhood of the fracture will result, and there is less likelihood of a deformity or of imperfect union at the point of the fracture. Whatever may be said of the fracture-box it is virtually an open dressing, and the surgeon and his assistants are constantly tempted to look and see if the fracture is doing well, with the result that the muscles in the neighborhood will be irritated and tend to contract. If it is possible at first to put a dressing on that will almost positively hold the bones in apposition and that will be permanent, that is the dressing to be preferred above all others; and should that dressing furthermore allow the patient to walk about, it is the ideal dressing.

Dr. Oscar H. Allis said that the oldest treatment on record and one that has never been reversed is, that in regard to fractures, there are two fundamental principles to be followed: first, to replace the fragments; second, to immobilize them. Dr. Martin has shown that these ends can be effected by the ambulant dressing as well as by the fracture-box. As to the important question of getting about, Dr. Allis expressed a fear that if a person had an ununited fracture or bad deformity following any new method of treatment, such as the one described, and were to go into the courts, the outlook for the surgeon would be rather dubious. The dressing of fractures with the ordinary plaster-of-Paris bandage has been long and favorably employed. The ambulant dressing need not interfere with this, as it may be made to take its bearing from a point above the fixation dressing. Dr. Allis further pointed out that in order for the patient to walk as conveniently as possible the foot is placed at right angles to the axis of the tibia; under these circumstances the stretching of the calf-muscles and the relaxation of others may result in overlapping of the fragments.

Dr. William J. Taylor said that if Dr. Martin had appreciated the amount of destructive change in the bones in the case specially referred to he would not have attempted treatment with the ambulant dressing. The case was not a suitable one; and the disease was very extensive, with overlapping of the fragments and a great deal of destruction of the bone. The patient was not seen until some time after the fracture. The site of injury was cut down upon and the bones were found very much displaced. The fragments of the tibia could not be brought together at all because the bone was broken at the lower end; consequently there was no possibility of good union. There was also a great deal of destruction of the tissues there.

Dr. James M. Barton stated that he had used this method of treatment in some four or five cases at the Jefferson Hospital. In none was the femur fractured, but in all the tibia and fibula were involved. The results have been fully satisfactory in every way.

In the first two cases some anxiety was felt, and the patients were kept in the wards during the entire treatment. The dressing was removed in the middle of the treatment, but the apposition being found satisfactory the dressing was reapplied. Four strips of rather heavy tin, roughened to prevent slipping, were used.

If the patient in stepping puts his weight on the toe, he will soon break the cast at the ankle. To avoid this, it is usual to bend the foot at the ankle, making the patient walk on the prominent heel. Some patients have found this position very irksome, and the dressing, when it is about finished, may be completed by putting a mass of cotton, about the size of the fist, under the heel and covering it with the plaster bandages. By this means the patient is slightly elevated; his toe cannot readily come in contact with the floor, and the foot is comfortably maintained at a right angle.

Dr. G. G. Davis said that, in most cases, the results of fractures are in direct proportion to the care exercised in treating them. The question suggests itself as to whether, in a difficult case, the means of treatment demonstrated would be as efficacious as other means. Dr. Davis believed not, although admitting that it was not brought forward as a means of treating all classes of cases. While the ambulatory treatment of fractures is a decided advance in surgery, and while progress in the treatment of fractures will be made in that direction, the question as to the best means of doing this has not yet been settled.

The next question is as to the time of application of the dressing. Shall a patient be immediately put upon an ambulatory treatment, or later? Personally, Dr. Davis believed that the best results will be obtained in nearly all cases by not putting the patient upon an ambulatory treatment until about eight or ten days after the bones have been broken, when the parts will have had time to glue themselves into suitable position.

Dr. Davis does not believe in the treatment of fractures solely by plaster-of-Paris, notwithstanding the assertion that plaster-of-Paris only gives unsatisfactory results in unskilful hands. It is an untrustworthy dressing to use from the first. Displacements can occur beneath the plaster, and these may remain unrecognized, to be discovered only upon the removal of the plaster. Therefore, it is preferable not to use any dressing that will prevent the surgeon from assuring himself that the fracture is in a good position. In some cases, in which the injury is comparatively slight, the tendency to deformity is almost *nil*, there being no displacement and practically no tendency for it to occur. Such cases will cure themselves under almost any plan of treatment. Ordinarily, the simple pain that is produced in walking or in attempting the use of a fractured limb will induce the patient to refrain and thus avoid appreciable displacement.

In the case of a patient under the care of Dr. Davis, with an undoubted fracture of the fibula and swelling of the ankle-joint, the limb was in a few days placed in plaster-of-Paris. Two weeks later pain was experienced, and on removal of the dressing it was evident that the tibia had been fractured obliquely through its outer half into the ankle-joint, and the traction of the tendo Achillis had drawn the heel upward and tilted this fragment forward. The position of the foot was apparently not changed in the plaster; it was only the pain that caused the dressing to be examined. Had this been a case of ordinary oblique fracture of the leg, the displacement would probably not have caused much pain, and the patient would have been allowed to have his leg remain in the splint, and the consequence would have been a deformity.

The field for ambulatory dressings for fractures is limited. While eventually the use of ambulatory dressings will become more general, and is in the line of progress, it is hardly as important as it is thought by some. It is comparatively easy to get brilliant results in children by the ambulatory method; so it is likewise by other methods. That a child can crawl around the floor with a fracture-box attached to its broken leg, is evidence of how little disturbance a fracture may cause. It has not been Dr. Davis' experience that a child's health runs down when it is kept in bed. On the contrary, children can live in bed almost indefinitely without their health deteriorating, and in tuberculous diseases at all events the health may improve. Therefore, the ambulant treatment is not so necessary with them. In many cases in adults there is no desirability of allowing a patient with a fracture of the leg, for instance, to use the affected extremity (the experience of the Germans has sufficiently demonstrated that the ambulant treatment of fractures of the thigh is not desirable, and the method is confined to fractures of the leg). The majority of people with fractures of the leg are not desirous, particularly in hospital cases, of at once resuming their active duties. Therefore, a certain period of recumbency or rest is not objectionable to them.

However, there are some cases in which it is desirable to get the patients up and about as soon as one possibly can, and in these a dressing such as that shown is an excellent thing. Other means of achieving the same object would be the use of a high shoe on the opposite foot and crutches, the support of the leg being made by a dressing of sodium silicate, or other immovable dressing.

The use of various forms of apparatus was likewise advocated some time ago, and more recently again by Bradford, of Boston.

Another very good way, and one of the most reliable ways, of achieving this object is by the use of the Thomas knee-splint, in which the support is taken upon the tuberosities of the ischium by parallel bars along the side of the leg. Surgeons are not yet qualified to state positively which is the best method of treating fractures; neither does the patient suffer much in the large majority of cases from his confinement on account of his fracture, but if the surgeon shows too great an anxiety to get rid, so to speak, of personal supervision of the case, in just that proportion will he be likely to find his results deteriorate.

Dr. J. P. Mann said that he has used the ambulatory method of treating fractures several times, and is not convinced that it is the best method of treatment for these cases. As to the use of plaster-of-Paris in the treatment of fractures, there is not a bone in the body, except the skull, that he has not applied that to. In *The Medical News*, July 15, 1893, p. 70, Dr. Hearn and Dr. Mann reported seven cases of fracture—four of the femur, two of the humerus, and one of the radius and ulna—treated with plaster-of-Paris and splints. There was in two of these cases a quarter of an inch of shortening. One of the cases was in an old lady, seventy-eight years of age, who fractured her femur just above the knee, making a compound oblique fracture. The wound was immediately asepticised, the bone replaced, and plaster-of-Paris put on from the toes to the arm-pit. The woman lay in bed for eight weeks with very little discomfort. The result is to-day yet perfect. In all of the cases reported the results were good.

There would be some danger in a case of oblique fracture of the tibia in the use of the ambulatory dressing. The amount of cotton used about the foot and ankle seems to permit more motion than is desirable, and overriding of the bone would very probably take place. In one of the children presented by Dr. Martin there seemed to be considerable backward curving of the tibia and fibula. In another there was considerable bowing outward of the leg. This, of course, might have existed before, although not evident in either of the unfractured legs. Of course, such results attend also other forms of treatment. It is preferable to use for all such fractures the usual plaster-of-Paris splint. In a fracture of this kind this splint is especially applicable because, during its application, extension and proper apposition of the parts can be very readily maintained. In case of a fracture along the shaft of the tibia, for instance compound, as that is the kind most frequently desired to be seen, a section of the plaster-of-Paris dressing is first placed above the seat of fracture and then another below the seat of fracture. Across the seat of fracture and along the sides of the leg are placed two strips of iron, which are incorporated between the layers of plaster. Thus the space can be bridged over, and then before fastening these lower two ends of the irons, which have been fastened above, an assistant makes extension upon the foot to put the bones in proper place. When the plaster hardens, it is impossible for the leg not to remain in extension. The wound is then readily accessible for dressing. Mostly, it is possible at once to cover the whole limb as far as necessary with plaster-of-Paris. If there is considerable swelling, absorbent cotton is used freely, and then, after the swelling has subsided, the first plaster dressing is removed and another put on that will fit more accurately.

No plaster-of-Paris dressing can be considered good unless it hardens within five minutes. Formerly, one of the strongest objections to plaster-of-Paris was that it took so long to harden. It was discovered some eight or ten years ago that if the materials used in plaster splints, as well as the materials used underneath the plaster, would not readily absorb water the plaster would harden slowly. Since absorbent lint and buttercloth (not cheesecloth) have been used it has been found that with a little salt put in the water, which should be slightly warm, the plaster can be made to harden within five minutes. In fractures in any part of the body away from the skull no dressing will do the work that plaster-of-Paris does. It is now being used extensively in hospitals, and as surgeons are becoming more accustomed to it they find no difficulty in its application. It can be removed very readily with an instrument devised by Dr. Reed, of the University of Pennsylvania. It was formerly removed with a penknife or saw, but Dr. Reed's instrument has greatly simplified its removal.

Dr. Edward Martin said that the possibility of producing deformity by bringing the toe up had occurred to him, but it did not happen to take place in the cases in which the dressing was used. The best dressing is that which does the work best.

As for the confinement in bed, it might be a good thing if all children could be kept in bed from the time they are five until they are twenty-one, but it seems scarcely a good thing to put children to bed if it is not necessary for their own comfort. It is rather better to keep them upon ambulant treatment if it can be done. If the amount of bone removed in the case operated upon by Dr. Taylor had been known, the ambulant treatment would probably not have been used. Under any circumstances it is doubtful whether the result was any better or any worse than would have followed any other method of treatment. The man returned to work, but, as often happens about the seat of old compound fractures, there developed apparently some necrosis one or two months afterward.

The material into which plaster is rubbed is a subject of indifference, provided that its meshes are of sufficient size—its absorbing power is dependent upon the removal of fat, a process to which most fabrics used in the preparation of plaster bandages are subjected.

THE LOCAL APPLICATION OF COLD IN ACUTE PNEUMONIA. BEING THE THIRD COLLECTIVE REPORT, WITH A DISCUSSION OF FEVER IN THIS DISEASE.

THOMAS J. MAYS, A.M., M.D.

[Read December 9, 1896.]

A little more than a year ago I had the honor of presenting to this Society my second collective report on the influence of cold in the treatment of pneumonia, which, with previous collections, comprised the histories of one hundred and ninety-five cases of this disease so treated, and this evening I beg leave to make a further contribution of one hundred and four cases to the same subject —making a total of two hundred and ninety-nine cases of acute pneumonia managed in this way. To the medical journals that again kindly published my late announcement inviting contributions, and to the medical gentlemen who so generously and liberally responded to my appeal, I desire to extend my grateful thanks. Of the one hundred and four cases, which are here considered, only ninety-four are reported fully enough to give the sex, age, highest temperature attained; highest number of respirations; number of days from rigor to crisis, or subsidence of fever; presence or absence of delirium; whether single or double pneumonia; recovery or death; and name and address of each reporter. In a few of the cases not all these data were furnished.

TABULAR VIEW OF NINETY-FOUR CASES OF ACUTE PNEUMONIA.

Number.	Sex.	Age.	Highest temperature.	Highest No. of respirations.	No. of days from rigor to crisis or to subsidence of fever.	Delirium.	Single or double.	Recovery or death.	REMARKS.	Names and Addresses of Reporters.
1	F	5½	105	.....	6	No	S	R	Well and out of bed on seventh day.	M. V. Ball, Philada.
2	M	4	104	.....	8	No	S	R	Very serious case; cyanosed; oxygen used.	" "
3	F	5	104	30	9	Yes	S	R	Recovery was prompt.	" "
4	F	8	104	.....	4	No	S	R	"In my cases ice was well borne, allayed cough, and recovery, as a rule, was very prompt."	" "
5	M	3	104.8	52	5	Yes	S	R	Catarrhal pneumonia following measles.	Chas. E. Woodward, West Chester, Pa.
6	M	3	105.2	50	3	Yes	D	R	Catarrhal pneumonia following measles; convulsions for twenty-four hours. Both of these cases were "promptly brought under control by the application of ice."	" "
7	M	27	104	40	2	No	S	R	Resolution progressive and permanent.	Henry E. Tuley, Louisville, Ky.
8	F	9	.....	....	2	Yes	S	R	Crisis occurred twenty-four hours after ice was applied.	R. E. Buchanan, Independence, Ia.
9	F	25	.. ....	. ....	.....	Yes	S	R	"I now use cold applications in all pneumonias and eruptive fevers, with uniform success."	" "
10	M	7	.....	.....	3	Yes	S	R	Relief was prompt.	" "
11	M	38	105.4	36	8	Yes	D	R	Very critical attack in an alcoholic and apparently hopeless case. Two hours' application of ice brought the temperature down from 105.4° to 101°.	P. F. Metz, New Haven, Conn.

Number.	Sex.	Age.	Highest temperature.	Highest No. of respirations.	No. of days from rigor to crisis or to subsidence of fever.	Delirium.	Single or double.	Recovery or death.	REMARKS.	Names and addresses of Reporters.
12	M	75	105.5	40	4	Yes	S	R	Third attack of pneumonia within two years.	P. F. Metz, New Haven, Conn.
13	F	36	104	32	5	Yes	S	R	As in the other cases ice relieved dyspnea, pain in chest and restlessness.	" "
14	M	10	104.5	44	4	Yes	S	R	Ice gave instant relief to pain in chest.	" "
15	M	58	105.5	40	4	Yes	D	R		" "
16	M	11	105	40	2	Yes	S	R	Discharged on sixth day.	" "
17	M	30	105.2	44	3	Yes	S	R	An alcoholic. Expectoration bloody at first.	" "
18	F	65	105.5	40	3	Yes	S	R	Reporter applied ice in a bag, mixed with saw-dust and covered with oiled silk.	" "
19	M	6	104	.....	.....	Yes	S	R	Pneumonia following measles.	A. H. Lindorne, Atlanta, Ga.
20	M	32	105	35	7	Yes	D	R	Case of alcoholic pneumonia with wild delirium tremens for three days and nights, which made it necessary to strap his four extremities to the four corners of the cot. The marked cyanosis was relieved by oxygen. Internally gave strychnin and capsicum in large doses.	Thomas J. Mays, Philadelphia.
21	M	35	103.5	32	8	Yes	S	R	A severe case.	Wm. A. Kimmil, Chicago.
22	M	14	104	.....	.....	Yes	S	R	Cloths, wrung out of ice water, applied to chest.	Geo. F. Dunham, Wenona, Ill.
23	F	2½	104	60	5	Yes	D	R	Had trouble in inducing family to use ice at first, but it acted "marvelously" when it was applied in this case.	Ira Haynes, Nashville, Tenn.
24	M	12	.....	.....	.....	.....	S	R	Asthenic case. Objections to the use of ice by family. These were overcome.	" "
25	F	10	.....	.....	5	No	S	R	Sister of last case. Same objections to ice. Cold reduced temperature in a short time.	" "
26	M	23	105	36	5	Yes	S	R	Treatment very satisfactory. Patient had pneumonia three years before, which lasted for two weeks.	" "
27	M	2½	103	.....	5	No	S	R	This case was a brother to the following one.	" "
28	F	5	103	60	6	Yes	S	R	Reporter says ice-treatment is as good for children as for adults.	" "
29	F	2¼	105	80	6	Yes	S	R	Case with "most unfavorable prognosis." Ice was not applied until the fourth day, and then as a last resort, and reporter believes "it saved the child's life."	G. H. Franklin, Hightstown, N. J.
30	F	10 mos.	104	.....	.....	.....	...	R	Patient became progressively worse under hot-poultice treatment. Ice was substituted and temperature fell from 104° to 102°.	Charles Rea, York, Pa.
31	F	76	.....	.....	.....	.....	...	D	Pneumonia complicated with influenza. Dr. Rea reports FOUR other cases (not tabulated) treated with cold, the ages of three of which were below eight, and one sixty years, of which he has no record in detail, but with recovery in all. Two of these were seriously complicated with pertussis. In conclusion he says that these cases "might have gotten well without the cold treatment, yet, apparently, I got better results *and my patients were more comfortable* than when I used other forms of treatment."	" "
32	M	4	105	60	6	Yes	S	R	Applications of cold to chest reduced the temperature and respiration-frequency immediately.	Anna M. Stuart, Elmira, N. Y.
33	F	14	106.2	40	5	Yes	S	R	Also pleural effusion. Aspiration.	H. G. McCormick, Williamsport, Pa.
34	F	58	103.5	36	3	Yes	S	R	Ice was applied when fever was highest, and the temperature fell at once and gradually came to a normal point.	" "
35	F	24	103.1	40	9	No	D	R	Case in a delicate girl. Ice gave decided comfort. In reporter's opinion recovery is due to the cold application.	J. H. Wilson and H. S. McConnel, New Brighton, Pa.
36	M	56	.....	.....	.....	No	D	R	Reporter says: "I most thoroughly believe in this form of treatment."	Theoda Wilkins, Pomona, Cal.
37	M	24	104.3	36	6	Yes	S	R	Ice was applied every four hours, and allowed to remain half an hour.	Charles A. Currie, Philadelphia.
38	F	38	102.5	.....	.....	.....	...	R	Acute pulmonary congestion. In bed two days. Pain, pyrexia and dyspnea were promptly relieved.	Arthur Dedoe, Seattle, Wash.
39	M	69	102.5	.....	.....	.....	S	R	Acute congestion right apex. Complication of serious aortic incompetency. Cold was substituted for hot poultices, which gave no relief. Patient was out in three days.	" "
40	M	6	104	.....	.....	.....	S	R	Reporter's son. Cold gave prompt relief.	" "
41	M	15	104.4	.....	6	No	S	R	In hospital nine days Good recovery.	J. C. Knipe, Jewish Hospital, Phila.
42	M	28	105.2	40	.....	Yes	S	D	Was in poor health with previous cough. Died on sixth day.	" "
43	M	38	102	.....	5	No	S	R	In hospital seven days.	" "
44	M	20	104.6	.....	4	No	S	R	In hospital eight days. Good recovery.	" "
45	M	20	104.8	.....	5	Yes	S	R		" "
46	M	33	103	32	7	Yes	S	R		" "
47	M	27	104.2	32	.....	Yes	S	R		" "

Number.	Sex.	Age.	Highest temperature.	Highest No. of respirations.	No. of days from rigor to crisis or to subsidence of fever.	Delirium.	Single or double.	Recovery or death.	REMARKS.	Names and addresses of Reporters.
48	F	17	104.2	48	.....	Yes	S	R	Very grave case. Was poulticed with hot applications for five days after the chill, but without influence on fever, delirium or restlessness. After this period ice was applied and the gravity of all the bad symptoms was markedly and almost immediately abated.	David F. Monash, Des Moines, Ia.
49	M	23	103.4	40	5	No	S	R	Cold gave relief to pain, dyspnea and restlessness.	" "
50	M	34	104.4	38	5	No	S	R	On account of friends' objection ice was not applied until third day. Benefit apparent at once. Patient addicted to alcohol and opium. Dr. Monash adds: "I strongly urged the use of cold in two other cases in another family, but neither the friends nor the attending physician would agree to it. One died, and the other recovered from the acute process, but merged into tuberculosis."	" "
51	M	60	103.8	40	6	Yes	S	R	Patient himself recognized the comfort derived from ice-bag. "I consider the use of ice in acute pneumonia of as great value as antitoxin is in diphtheria."	W. J. Fletcroft, Paterson, N. J.
52	F	36	104	40	7	No	S	R		" "
53	M	16	104	40	5	No	S	R	This patient, although he made a good recovery, had another attack of pneumonia three months later. (See next case.)	" "
54	M	16	105.4	40	7	No	S	R	Until the fever had risen to 105° on the fifth day the patient was treated with hot poultices. After this ice was applied and the fever dropped at once.	" "
55	M	25	104.4	40	4	Yes	S	R	Ice was applied on the fourth day and in twelve hours crisis occurred. Patient had insomnia from time of chill until ice was applied. In his delirium he jumped out of second-story window. Recovery was slow.	" "
56	F	36	.....	.....	.....	Yes	S	R	Patient was not seen until four days after chill, when ice was applied. Good recovery.	Geo. S. Stone, New Freedom, Pa.
57	M	10	.....	.....	.....	No	S	R		" "
58	F	18 mos.	.....	.....	.....	No	S	R	The last three cases were in mother and two children, the attack in the first beginning on March 8, and that in the children on March 10, 1896. The two ended by crisis and the first by lysis.	" "
59	M	78	.....	.....	2	No	S	R	Pneumonia ran its course in forty-eight hours.	" "
60	M	22	.....	.....	.....	No	S	R	Patient improved from time ice was applied.	" "
61	M	40	.....	.....	.....	Yes	S	D	Patient addicted to alcoholic excess. In an intoxicated state, he lay a whole night on the frozen ground. Unconscious most of the time.	" "
62	M	20	.....	.....	4	No	S	R	Ice gave immediate relief.	" "
63	M	40	104	.....	.....	Yes	S	R	Patient alcoholic. Expectorated much blood.	" "
64	M	20.	104.5	.....	.....	Yes	S	R		" "
65	F	38	105.5	.....	.....	Yes	D	R	Whole chest enveloped in ice. Patient begged for ice when it was removed. "Am confident the ice saved her life."	" "
66	M	8	106	.....	9	Yes	S	R	Temperature fell directly after ice was applied.	C. B. Bastian, Salladasburg, Pa.
67	M	6 mos.	106	.....	.....	.....	S	R	Pneumonia complicated with convulsions.	" "
68	M	12	104	.....	.....	Yes	S	R		" "
69	F	6	.....	.....	.....	Yes	S	R	Reporter believes ice limited the extent of inflammation.	" "
70	M	20	104	.....	.....	Yes	S	R	Reporter gave case up as hopeless. Made a slow recovery.	" "
71	M	10	105	.....	.....	Yes	S	R	Pain so severe that opiates were required.	" "
72	M	8	.....	.....	.....	No	S	R		" "
73	M	46	103	.....	.....	Yes	D	R	Different areas were successively invaded. In bed three weeks.	" "
74	M	10	104	.....	.....	Yes	S	R	"Ice always lowered the temperature."	" "
75	F	70	103	.....	.....	Yes	S	R	"Think ice saved this patient."	" "
76	F	6½	105	44	.....	No	S	R	Reporter states that he is a firm believer in the efficacy	J. R. Care, Worcester,
77	F	27	104	36	.....	Yes	S	R	of the ice-treatment of pneumonia; and adds, "I	" " Pa.
78	M	16	105	36	.....	Yes	S	R	regret very much that, owing to loss of my notes, I am unable to give the history of HALF A DOZEN other desperate cases (not tabulated) treated in the same way, all of which, except one, I think owe their lives to the cold treatment."	" "
79	M	28	103.5	28	3	No	S	R	This case began with great dyspnea and pain in the right lung. The ice relieved these symptoms.	B. F. Kierulff, Marshalltown, Ia.
80	M	9	105	30	3	Yes	D	R	Ice was also used internally with great benefit.	" "
81	F	40	104.2	50	8	Yes	D	R	This was a critical case of pneumonia in which an unfavorable prognosis had been given by the attending and consulting physicians; and while passing through the attack the patient gave birth to an eight-months' fetus. Her delirium was violent, sometimes alternating with muttering and subsultus tendinum, but after the ice application, all these grave symptoms gradually subsided. The ice was used as "a forlorn hope."	A. H. Boyer, Philadelphia.

Number.	Sex.	Age.	Highest temperature.	Highest No. of respirations.	No. of days from rigor to crisis or to subsidence of fever.	Delirium.	Single or double.	Recovery or death.	REMARKS.	Names and addresses of Reporters.
82	M	50	105	50	10	Yes	S	R	This was a very aggravated case and had been abandoned by another physician. Reporter thinks the ice and strychnin saved him.	A. Chas. Dogge, Helena, Mont.
83	M	38	105.5	60	5	Yes	D	R	A very unfavorable case, suffering with chronic articular rheumatism and endocarditis.	" "
84	M	13	104	50	9	Yes	S	R	Dyspnea great; face cyanosed, but so soon as ice was applied improvement occurred.	" "
85	M	21	104	.....	.....	Yes	D	R	Ice of great benefit here.	" "
86	F	35	104	40	.....	Yes	S	R	Ice aggravated dyspnea, pain and restlessness. These symptoms were relieved by the administration of small doses of hydrogen dioxid.	" "
87	F	26	.....	.....	.....	Yes	S	R	Followed an attack of typhoid fever.	" "
88	M	46	103.5	.....	.....	Yes	S	R	At first objected to ice, but finally consented to its use. Result very good.	" "
89	M	40	105.5	50	.....	Yes	S	R		" "
90	M	20	104	45	.....	Yes	D	R		" "
91	M	35	.....	.....	.....	Yes	S	R	At the same time these cases were treated, reporter had another case of pneumonia in which, because he was not permitted to apply ice, he used hot poultices, and the patient died.	" "
92	F	25	105	.....	.....	Yes	D	R		" "
93	M	30	105	40	.....	Yes	S	R		" "
94	F	47	102	48	7	.....	S	R	Marked orthopnea and cyanosis. Twenty-four hours after ice was applied, the cyanosis had almost entirely disappeared and the patient was able to lie down and sleep; her respirations had fallen from 48 to 24, and her pulse-frequency from 110 to 90.	Charles A. Currie, Philadelphia

Sex.—The sex is noted in ninety-four cases, of which sixty-three are males and thirty-one are females.

Age.—The age is also given in ninety-four cases. The number occurring in each decade is as follows: Under one year, 1; between one and ten years, 22; between ten and twenty, 16; between twenty and thirty, 20; between thirty and forty, 16; between forty and fifty, 7; between fifty and sixty, 4; between sixty and seventy, 3; between seventy and seventy-five, 2; between seventy-five and eighty, 2.

Highest temperature-range.—This is given in seventy-six cases as follows: In two, 102°; in two, 102.5°; in six, 103° and 103.5° respectively; in twenty-four, 104°; in seven, 104.5°; in seventeen, 105°; in nine, 105.5°; in three, 106°.

Highest respiratory frequency.—This is noted in forty-seven cases. In one it was 28; in two, 30; in four, 32; in one, 35; in six, 36; in one, 38; in sixteen, 40; in three, 44; in one each 45, 52, and 80; in three, 60; in two, 48; and in four, 50.

Days of crisis or subsidence of fever.—This is noted in fifty-two cases. In four this occurred at the end of the second day; in seven, at the end of the third day; in seven, on the fourth day; in thirteen, on the fifth day; in seven, on the sixth day; in five, on the seventh day; in four, on the eighth day; in four, on the ninth; and in one on the tenth day.

Number of cases of single and double pneumonia.—There were seventy-six cases of single and fifteen of double pneumonia.

The mortality.—Out of the one hundred and four cases, three died—giving a mortality-rate of 2.88 per cent. The ages of those who died were 28, 40, and 76 years respectively. Out of the total number of 299 cases so far collected there were ten deaths—giving a total death-rate of 3.35 per cent.

FEVER AND ACUTE PNEUMONIA IN THEIR RELATION TO COLD EXTERNALLY APPLIED.

As fever is such an important feature of pneumonia, and as I believe that a correct interpretation of the pathologic relationship between the two is indispensable in order to formulate principles for the correct treatment of both, I desire to take advantage of this opportunity to briefly discuss the general nature and pathology of fever, and afterwards, in connection with other neighboring lesions, to consider its special bearing on pneumonia from the therapeutic standpoint that is here advocated.

What then is the essential nature of fever? Does it make for health or for disease? Is it a process of evolution, or of dissolution? Does it build up or destroy? Is it an intrinsic evil, or is it a thing to be welcomed and encouraged within certain limitations in the hope that it will enable nature to fight and to overcome disease; or does it make the last state of the body worse than the first? The only rational answers that can be given to these questions are to be obtained through a study of the production of the normal temperature of the body, and this leads to the following questions: Where and how is the normal heat of the body produced and maintained? Where is the fire, and on what does the fire feed? Physiology teaches that the heat of the body is almost exclusively produced through oxidation, and that four-fifths of this amount comes from oxidation occurring in the muscles. In other words the muscles contain something that on being oxidized furnishes nearly all the heat of the body; or, a part of the body is burnt up for the purpose of supplying its own temperature.

Now, while heat is being thus generated there is at the same time another process in active operation, viz., that of heat-loss, or heat-dissipation. This serves as a heat-leveler and tends to limit heat-accumulation in the body. From an estimation of these two factors it is evident that the degree of temperature present in the body at any moment is dependent on how much is produced and how much is lost at that time; or, in other words, the degree of body-temperature at any time is the balance between heat-production and heat-dissipation. We are told that in the early days of railroad-engineering it was impossible to operate a locomotive in rainy weather on account of the cooling effect that the rain had on the steam-boiler. Dissipation of heat became too active here, and the locomotive was disabled in consequence. It is evident, therefore, that in order to preserve the function of the machinery in the human body or in the locomotive, neither heat-production nor heat-loss must go on without restraint; and that both must be adjusted in such a manner that no more heat is produced or lost than is necessary to carry out this purpose. This is exactly the case in health, for here the temperature is almost absolutely constant and uniform because it is under the control of a specialized nervous apparatus known as the thermotaxic mechanism, which regulates both heat-production and heat-dissipation. When the function of this mechanism becomes deranged, or is overthrown, either heat-production or heat-dissipation, or both, become disordered, and fever, or hyperpyrexia, or in some instances a subnormal temperature may supervene.

The history of the locomotive furnishes an appropriate prototype of the development of this thermotaxic apparatus. In the crude way in which the former was at first constructed no provision was made for preserving a normal relationship between heat-production and heat-loss; but by degrees this difficulty in construction was overcome, and in the machinery of the present locomotive there is established such an adjustment between heat-production and heat-loss that it may be operated under conditions that formerly made this impossible. In the lower forms of life, as in the case of molluscs and reptiles, the thermotaxic mechanism is but poorly developed, and the animal's temperature, like that of the early locomotive, is at the mercy of the surrounding temperature. The temperature of a warm-blooded animal poisoned with curare, like that of the cold-blooded animal, corresponds in rise and fall with the temperature exterior to the body—the influence of the thermotaxic mechanism is entirely incompetent to regulate the bodily temperature, because curare paralyzes the peripheral nerves. In the first days of extra-uterine life the temperature is subject to wide variations—showing that at this period of life the thermotaxic mechanism is excessively unstable. Thus it is plainly seen that the development of the function of heat-coördination in animal life runs in a line parallel to that of the perfection of the steam locomotive. In the primitive history of both, heat-control is unsteady and defective, while in their

higher stages of evolution this function has attained a great degree of stability and perfection.

It has already been stated that the nature of the thermotaxic mechanism is nervous and it is of interest to know that its anatomic seat is pretty clearly defined. According to Ott, Aronsohn and Sachs, Eulenburg, and Landois, there are six heat-regulating centers—two in the cortex, and four at the base of the brain. These experimenters have also shown that electric or mechanical irritation of these centers produces a rise of temperature lasting for hours at a time.

From all that has been said it is quite evident that Finkler is correct in describing *fever as a neurosis*, or more specifically, fever is due to disturbance of the thermotaxic mechanism and can in no sense be looked upon as a healthy reaction of the organism against the cause of disease. One might as well view the bending, the twisting, the creeking and the crackling of forest trees in a tornado as evidence of a normal reaction against the fury of the storm. In one sense it is a reaction of those that survive, but more correctly it is a contest that shakes and shatters and threatens the life of every tree that is attacked, and in which those that fail to possess the requisite resistance go down, while those that weather through it, perhaps, never fully recover from its effects.

Fever is always evidence that the function of heat-coördination is disorganized and has lost its power of restraining heat-production or heat-dissipation, or both, and is *per se* a morbid process. Fever may be compared to a heart whose beats run wild when certain cardiac nerves are irritated or disturbed. The heart, under these circumstances, does not beat with greater rapidity because there is greater resistance to be overcome in front, nor is its strength enhanced, but it behaves in this manner because the restraining influence of some important nerves is crippled. So, fever is a manifestation of depreciated nerve-integrity, and never serves the interests and purposes of health. Not only is fever an ordinary disease, but it has been shown by Vincent that the blood of animals that die from the effects of artificial fever contains a poison that causes convulsions, stupor and death, in guinea-pigs, sparrows and frogs.

From these brief considerations it is quite evident that fever is caused by disorder of the heat-centers—the principal ones of which are located at the base of the brain; and it is therefore of great interest to bear in mind the close anatomic relation of these centers to another important center which is located in the same region and which has a special affiliation with respiration. It is well known that a dog pants to cool himself off through the lungs, and that a fevered animal breathes rapidly in order to increase the rate of heat-discharge through the same channel. Richet, Ott, and other physiologists believe that the rapid breathing under these circumstances is due to the fact that fever-heat stimulates an independent nerve-center at the base of the brain which has the power of accelerating the respiration-rate, and which they call the thermo-polypneic center. Other observers hold that this phenemenon is brought about through the instrumentality of the respiratory center, which is stimulated to increased activity by the same cause, *viz.*, fever-heat. I believe that the evidence in favor of the existence of a special polypneic center is well made out; but whether it is or not, or whether the respiratory center, which is also located at the base of the brain, subserves the function of accelerated breathing during fever, it is clear enough that an increased respiration-frequency is always associated with acute pneumonia. So, too, there are also very good reasons for believing—reasons that I have discussed in my second collective report on acute pneumonia—that the process of pneumonia itself is in a great measure dependent on serious disturbance of the nervous system in which the respiratory nerves and center participate very largely, and hence the increased frequency of breathing in this disease may be due partly to central and partly to reflex influences. Moreover, in close anatomic contact with the

respiratory, the polypneic and the heat centers, is the convulsive center, and in this close companionship we have, perhaps, an explanation why almost all inflammatory diseases that implicate the base of the brain are attended with a frequent respiratory rate, high fever from the onset with little fluctuation, together with convulsions occurring with greater or lesser frequency. Pneumonia and cerebro-spinal meningitis are prominent examples of such affections, especially when they occur in the infantile period of life. The reason why this combination of symptoms is developed more frequently in the young than in the old is because as age advances the various nerve-centers become less impressionable and more independent of each other, and hence are less easily thrown out of equilibrium by incidental disturbances. Then, beside the three kinds of centers already spoken of, the oblongata contains another one, *viz:* the cardiac center, which is invariably involved in pneumonia, and plays a very prominent *rôle* throughout the course of this disease.

In thus analyzing some of the important pathologic factors in the nervous mechanism of acute pneumonia the therapeutic indications in this disease are brought out very prominently. These resolve themselves into (1) an abatement of the local process in the lungs; (2) a reduction of fever, and (3) an alleviation of cerebral and general nervous irritation. Now, how are these extensive indications to be met? What is the remedy, or does it require more than one? After due consideration of all the clinical and experimental data so far as I know them, I believe that cold applied externally will meet these demands more satisfactorily than any other measure. Not only will it allay the morbid process in the lungs, but it possesses a direct ameliorating influence on those nerve-centers which, as we have seen, play a responsible part in the pathologic history of acute pneumonia.

EFFECTS OF COLD APPLICATIONS TO THE CHEST.

Cold, when applied to the chest, has a two-fold effect. First, it limits and checks the extension of the pneumonic process in the lungs and assists in bringing about resolution. This is borne out by the observation of any practitioner who has carefully watched the stoppage and the recession of this process as indicated by the modification of the physical signs. Secondly, through its action on the respiratory and the peripheral nerves of the thorax, cold has a profound sedative reflex effect on the heart, the respiratory and the polypneic centers—affecting these to almost the same degree as if it were brought in direct contact with them. Experiment confirms this by showing that when in an artificially fevered animal a piece of ice is applied directly to the polypneic and heat centers, as has been done by Ott, the respiration falls in frequency and the fever is promptly reduced. Clinical experience demonstrates that this is precisely what happens when cold is applied to the chest in this disease. The respiration and pulse are reduced in frequency; the fever abates, the temperature sometimes falling several degrees in a very short time; the day of crisis is hastened, and if the attack is not aborted, its duration is materially shortened.

COLD APPLICATIONS TO THE HEAD.

Cold applied to the head acts directly on the brain and on the implicated centers at the base of the brain. Stress should be laid on the fact that such applications are always to be made in cases with high fever, great restlessness and general irritability, and we should, at the same time, remember that they are not out of place in less severe cases. They materially assist and reinforce the effects of the cold that is simultaneously applied to the chest.

METHOD OF APPLYING COLD

The important practical questions that confront us are: How is cold to be applied and how long is it to be continued? In my practiee it is employed in the form of pieces of ice placed in rubber ice-bags. Others use snow, either in rubber bags or wrapped up in towels; or apply towels wrung out of cold water. The bags may be wrapped in towels, and, after they are applied over the inflamed area, a broad, thin bandage may

be placed around the chest in order to keep the bags in place. This is of special importance when the patient is restless and tosses about the bed. If the disease is confined to the front base on one side,only one good-sized bag will be necessary; but if the exudation extends higher up, or on the side or on the back, then one or two more bags must be applied laterally and as far back as possible. If the affection is extensive, as many ice-bags as are necessary to cover the whole area must be put on. I have had as many as eight ice-bags on the chest at one time. If the morbid process migrates, as it frequently does, it should be followed up with the ice and covered.

The length of time for which cold is to be used must, in most cases, be largely determined by the amount of fever present. If the temperature falls to or near the normal point, and shows a tendency to remain there, then the ice may be gradually removed. It is best, however, not to be in too much haste in withdrawing the cold, for frequently, when this is done, the temperature soon flies up again suddenly, and under these circumstances it seems to be more difficult to bring the temperature down than it was the first time. Sometimes, too, the temperature rises suddenly after the ice has been taken off, not because the disease has become active again in the old area, but because it has extended to a contiguous field of lung-tissue. It must be borne in mind, however, that the ice is not employed solely for the purpose of reducing fever, but also with the object of circumventing the exudative process and of hastening resolution. There may be very little fever present in some cases of pneumonia, yet the destructive changes are going on in the lungs at a rapid rate. Hence in senile, latent, or alcoholic pneumonia the activity with which the ice is employed must be governed by the impression that is made on the pulmonary disintegration.

Fever in pneumonia must, therefore, be regarded as an entity of powerful import, and one that demands special treatment. It is true that fever is not as simple and as gross a phenomenon as that which obtains in the local lung-lesion; yet it must be remembered that the latter is not the fundamental lesion from which the patient is suffering, but is merely the manifestation of some deeper or less apparent disorder. Just as pain, which is evidence of an irritated sensory nerve-center, is amenable to the influence of morphin, so is fever, which is the open and visible sign of some profound perturbation of those centers which coördinate bodily temperature, also to be abated and alleviated by the local application of cold.

DISCUSSION.

Dr. James Tyson said that his own experience in the treatment of pneumonia with cold has been rather limited, but so far as it has gone it has been satisfactory, so satisfactory indeed, that he has concluded for the present, at least, to treat, in this manner, all cases of pneumonia associated with high temperature that may come under his care. The attempt to arrive at a rational explanation of the operation of cold in these cases seems to be attended with difficulty so great as to make it scarcely worth while to spend much time upon it in a brief discussion. It is well known that the fever is a symptom secondary to some potent cause, such as a vegetable organism or a toxin generated by it, while the fever is an index in some way of the seriousness of the operation of this cause. It is also likely that the treatment by cold is useful in pneumonia exactly as it is in typhoid fever, while the high temperature itself, being less prolonged, is *per se* less dangerous. The most desirable stage to reach in the practice of this matter is to get over the fear of any possible harm to the patient from the cold, for such fear is entirely unfounded. It is not at all a new doctrine that it is impossible to do harm by cold to a patient who is in a state of fever, as long as the fever lasts. It matters not much how this cold is applied,whether by cold baths or by opening windows and allowing the cold winter air to blow upon the patient. There is a certain advantage in treating pneumonia by the direct application of cold to the chest. One other point of great importance in this treatment is to discontinue the cold as soon as the temper-

ature approaches the normal. It is also well not to use the cold in those cases, especially met with in old persons and in alcoholics, in which there is no elevation of temperature.

Dr. J. M. Anders said that in the discussion that followed the reading of Dr. Mays' former paper on the same subject he stated that he had used ice locally in a couple of instances of pneumonia with encouraging results. To that experience he has added half a dozen cases, in all of which the effect of the cold was also very good. In no case did the cold applied locally abort the disease, but the general course of the cases seemed to be somewhat milder than under other methods of treatment. The temperature ran a lower course and in three of the cases it seemed that the area of consolidation remained more distinctly circumscribed than in cases otherwise treated, although this may have been a coincidence. Dr. Anders has not, however, used cold locally as a routine measure; nor did he agree that cold is applicable to all cases of acute pneumonia. The cases so treated were selected. The patients were not infants, nor very old persons; they were not individuals who had been previously much debilitated or who possessed markedly sensitive organizations. If the advice laid down by Dr. Mays were followed rigorously accidents would be common. Occasionally instances will be encountered in which the cold will do more harm than good. Dr. Anders referred to a case of lobar pneumonia seen in the practice of a colleague in which neither the poultice of ice nor cold compresses could be tolerated. The patient was a lady about sixty-two years of age and seemed to have a very highly sensitive nervous system.

In all cases of pneumonia, particularly in the first stage, or that of engorgement, there is apparently a clear indication for the use of cold locally, as it is known that this agency causes contraction of the dilated capillaries and thus relieves or scatters congestion even. Now, whilst admitting these facts, and whilst believing that in the vast majority of cases cold should be used, still it should not be employed as a routine measure.

Dr. H. A. Hare agreed that cold should be employed in certain selected cases of pneumonia; there is not a single measure that can be employed as a routine means of treatment in every case, and the difference is very marked between the application of cold in a disease like typhoid fever, and the application of cold in a comparatively short-lived disease, such as croupous pneumonia. Physicians, when called upon to treat a case of croupous pneumonia, are inclined to be therapeutists instead of being watchmen, and to use remedial measures all the time rather than when indications for them arise. After the stage of congestion has passed and the second stage has been entered upon, indications must be met and combated as they arise. The damage is then done, and we must aid in clearing up the wreck. A temperature of 103° or 104°, which only lasts for a day or two, does not exert any particular deleterious influence upon the patient. If it goes very much higher than this and comes to be a hyperpyrexia, then the question of heat-stroke must be considered. Below this, it is simply a manifestation of the disease, and the fever does not call for medication by drugs or treatment by external remedies. It is not likely that cold, locally applied to the chest, yields all the good results that have been claimed, over and above that of cold in the reduction of fever. In the early stage of pneumonia an attempt should be made to relieve congestion by appropriate measures, but if the lung is wrecked by exudate, it seems that cold will be of little service. On the whole, when the stage of consolidation is very well developed, the continuous application of cold would not necessarily result in resolution and absorption, and it is necessary to treat with care cases of pneumonia in which, either because of inherent feebleness or delicacy of the system, cold is contraindicated. The continued application of cold to the patient's chest, up to the time of crisis, may do exactly what the physician so anxiously tries to avoid, *i.e.*, produce a sudden fall of temperature from 104° to below normal, with the patient almost or entirely collapsed. One of the most important things that could be done would be to discover the limitation of the usefulness of cold to certain cases of pneumonia possessing certain peculiarities, and to see whether it is not possible, by carefully going over the statistics, to limit the application of cold to some particular stage of the disease. It can be imagined, for instance, that when pericardial complications attend pneumonia, an ice-bag might do good, just as it does good in some cases of ordinary uncomplicated pericarditis.

Dr. S. Solis-Cohen said that if statistics might be relied upon to prove anything as to the treatment of any disease by a special method, the statistics presented would at first seem to prove Dr. Mays' contention, but cases of both croupous and catarrhal pneumonia differ so much in origin and in course, as well as in the complications with which they are attended, in the age and constitution of the patient, in the time and circumstances under which they are manifested, that it would be utterly impossible for any one plan of treatment to answer in every case, any more than one could expect a coat, no matter how good its texture or how beautiful its fashion, to fit every person in a large gathering. On the other hand, the statistics of Fenwick from the records of the London Hospital, which include one thousand cases of acute lobar pneumonia carefully studied, and in which all the concomitants, the ages and comparative vigor of the patients, the general and special symptoms of the cases, the complications, the diseases, if any, upon which the attack of pneu-

monia has supervened, the time of year and other modifying circumstances, have been considered, show one thing clearly and that is the usefulness of fever and fever-heat in pneumonia as Nature's method of bringing about recovery. It appears from these statistics that thirty-five per cent. of all deaths coincided in time with the sudden deferescence, the only period more fatal than this being that of the height of the fever. Those cases attended with an average temperature of 103° F. presented the minimum of mortality, the states of apyrexia and of hyperpyrexia corresponding with the maximum of danger to life.

In other words it is the extremes that are to be dreaded, whether of high temperature or of low temperature. These views are not novel or on trial. They are the old Hippocratic doctrine stated in modern language, and it seems that this doctrine, namely, that certain elements of the febrile complexus, and especially the febrile heat, being the reaction of the normal organism against the invading hosts of disease, are to be controlled but not antagonized, is thoroughly established. Notwithstanding advances in laboratory methods a great deal is still to be learned from the old clinical observers who studied their cases carefully and who took note of all of the environing circumstances, while with all the instruments of precision there exists the liability to overlook important facts. Careful reading of the teachers of medicine throughout all time will show that this doctrine of the usefulness of fever as Nature's reaction against causes and processes of disease has been held by those whom we may properly regard as leaders of medical thought, and none of the recent exact investigations has tended in any degree to overturn that doctrine. It may not be easy to understand it or satisfactorily to explain its mechanism, but it is an absolute clinical fact.

It would be preferable to see a patient with lobar pneumonia run along with a temperature a little higher than one a little lower than the average. One could be more hopeful of a favorable result. It would indicate not so much the severity of the disease as the energy of the salutary reaction. On the other hand hyperpyrexia is unquestionably an indication of danger; it shows not an energetic reaction, but an unbalancing of the regulatory mechanism, perhaps as a result of profound intoxication, perhaps of constitutional weakness—but these are speculations upon unsolved mysteries. Dr. Cohen has seen a number of cases of pneumonia in which cold applications have apparently done good; a few in his own practice, more in the practice of others. He has seen some of Dr. Mays' own patients at the Polyclinic Hospital, which seemed to do remarkably well under the influence of local cold and who seemed to be pretty comfortable with it. If any general rule in these cases could be laid down it would be to use cold applications whenever they added to the comfort of the patient. They would then be likely to prove useful; but whenever they added to his discomfort they would be likely to prove harmful. Hot local applications in pneumonia have been used from time immemorial; and are to be preferred, if they seem to give relief. The same rule applies and the effect is probably very similar.

Dr. Ernest Laplace maintained that the pneumonia produces fever, or that fever in pneumonia is a direct result of the amount of toxin remaining in the blood, because of the development of the pneumococcus in the blood, and that there is fever in a direct ratio to the toxin present. This toxin is a chemic substance and it passes through the blood to the thermogenic centers at the base of the brain. These centers are irritated and the result is fever. That is no mystery, but a scientific fact which can be reproduced, not with the toxin of pneumonia very well, because that is beyond any crude analysis of the chemist to-day; but pneumonia with fever can be produced by any chemical at will, say with compound tincture of iodin, as a direct result of the irritation of the base of the brain. Most violent diseases, whether surgical or medical, accompanied by high fever, have a tendency to recovery and those which are accompanied by less fever have less tendency to get well. In other words, when there are great disturbances of the body, the resulting variations of temperature will so alter the soil of the system as to render that soil itself deadly to the invading germs and they will die spontaneously and the patient gets well. Tuberculosis of spinal origin or of the hip-joint is at no time attended with the production of sufficient poison to alter the soil and make that soil itself death-dealing to the germ, as in pneumonia. As a result the body is slowly worn away and the patient dies. On the other hand the tetanus-bacillus develops quietly, but its toxin passes through and irritates the brain; it acts like strychnin, which, unlike compound tincture of iodin, will not induce fever, but simply causes violent spasm and as a result the soil is at no time death-producing to the germ. In other words, the progress of pathology, medical or surgical, is directly in the line of the reaction of the tissues of the brain, or of other organs, to the products of micro-organisms and the same resistance of the tissues of the body to the various toxins exists as between the tissues of the body and various drugs used.

Dr. J. C. Wilson expressed regret that in the collection of cases reported and their analysis the distinction between croupous pneumonia and broncho-pneumonia—two entirely distinct diseases—had not been made out. He emphasized the statement that the height of the temperature is no index of the severity of the process. Well-developed croupous pneumonias, beginning abruptly with high temperature,

run, on the whole, a course usually more favorable than those in which the temperature does not rise above 102.5° F.

Dr. Wilson has been in the habit for some years of using applications of ice to the chest in the manner described by Dr. Mays, in the treatment of croupous pneumonia both in private and hospital practice. There is no reason to suppose that the application of cold to the chest has any direct action upon the blood-vessels of the interior of the lung. It is certain, however, that it relieves pain to some extent, as well as lowers the respiration and to a trifling extent reduces the temperature. There is no ground for believing that it at all limits the extent of the exudate nor has a most careful study of the subject shown that it hastens the crisis.

Dr. J. P. Crozer Griffith said that the treatment of pneumonia with ice is no new one. He watched it years ago in Vienna and was impressed by the fact that it relieved pain, but did not influence the progress of disease in the slightest degree.

Dr. M. V. Ball said that in croupous pneumonia in children between the ages of four and twelve he has found the cold treatment very useful. How it acts is uncertain, scarcely by limiting the course of the disease, although it does seem to relieve pain, and children sleep well under it, while there is no difficulty in applying it.

Dr. Thomas J. Mays said, in regard to the differentiation between catarrhal and croupous pneumonia as they stand related to the treatment with cold, that in his earlier experience with the ice-bag he confined it exclusively to cases which were affected with the croupous variety of this disease. He soon found that others, among the first of whom was Dr. Franklin, of Hightstown, New Jersey, used it with equal success in the acute catarrhal variety following measles, etc., and since then Dr. Mays has used it in all cases of acute pneumonia, unless there existed some possible contraindication.

It has been well said that fever is no index as to the severity of the pneumonic process, although practically this point does not receive general recognition. Thus there may be very high fever with very little lung-involvement, and on the contrary, a large area of pulmonary disturbance with little or no fever. It must be remembered, however, that the degree of temperature present may not always represent the degree of thermotaxic disturbance.

Concerning the influence of cold on the pneumonic process it is very difficult to say anything definitely so far as the *modus operandi* goes, yet every observer will bear testimony that it possesses a direct influence on the local process. This was the experience of Dr. Lees, of London, who wrote on this subject before Dr. Mays did, and from whose work the latter received the inspiration which led him to test this treatment; and it has been seen over and over again by a number of other practitioners. It not only limits the pneumonic process, but in many cases when it is applied early enough it cuts short the disease.

In regard to the danger of fever Dr. La Place hit the nail on the head when he said that pneumonic fever develops the toxins referred to in the paper, and which have the power of producing convulsions, stupor, and other morbid nervous manifestations in the lower animals. Is it wise, therefore, to sit passively by and allow these agents to disintegrate the nervous system as far as they are capable of doing so, or is it not the physician's duty to make every possible effort to reduce the fever and thus check and annihilate this process of devastation? As has been said, it is well known that a temperature of 104° is not dangerous, at least not dangerous to life in most cases, yet to say this seems to be somewhat apologetic; for most physicians welcome a fall from 104°, rather than see the temperature remain at this point, and if intelligent efforts fail in bringing about a reduction, the fault does not lie so much in the physician as it does in the disease. While fever may be a good thing for the disease it is a very poor and dangerous experiment for the patient.

Nor does the coat-fitting argument apply to this case; for no one advocates the indiscriminate use of cold in pneumonia. There are certain characteristic morbid conditions which are known and recognized as pneumonia, and while it is true that cold does not fit every case in which such a collection of symptoms obtains, yet those experienced with cold know that it entirely fits the great majority of such cases; and those in which it is not admissible must be regarded as exceptions to the general rule. The same objection may be leveled against the external use of cold in typhoid fever; yet as a rule no one at present thinks of treating this disease without the use of cold-water baths.

The other objection that has been raised that the local is not superior to the general application of cold in pneumonia certainly gainsays the experience of Dr. Mays and that of every one else who has made a comparative study of this question. That general bathing is useless in this disease is not asserted, but when it is desired to get the best effects of cold in pneumonia its influence must be concentrated on the local condition in the lung.

A UNIFORM STANDARD OF MEDICAL EDUCATION FOR THE DEGREE OF DOCTOR OF MEDICINE.

SILAS UPDEGROVE, M.D.

[Read December 23, 1896.]

The State recognizes an educational requirement for the physician, has colleges to confer the degree of Doctor of Medicine and accepts the possession of a degree as *prima facia* evidence of the possession of the medical knowledge necessary for the competent physician. In order that those only who have the necessary medical knowledge shall be accepted as physicians, the State must have a recognized medical authority as to what is to be accepted as medical knowledge. In the absence of such an authority, however, degrees are being conferred upon an educational basis at one college which at another is rejected, and that which is a contradiction of the former is substituted as the basis of the educational requirement for the degree in the latter. As the basis of the educational requirement of one is a contradiction of that of the other, they cannot both be correct; nor can the possession of a degree in each afford evidence of educational competency. Notwithstanding this fact, during more than half a century at a college in this city of a so-called medical sect a degree has been, and is still being conferred, with a therapeutic formula as the basis of the educational requirement; the possession of this degree, in the absence of a recognized medical authority, being accepted by the State as evidence of educational competency: while the medical profession of the world declares that a universal therapeutic formula has not been scientifically established and does not truthfully exist. Under these contradictory methods the degree in one case must have no value and the State cannot have competent physicians until an authority is recognized as to what is to be accepted as medical knowledge.

Singular as it may seem, a decision by the courts as to the question of medical authority has never yet been called for, or the medical profession of the world must surely have been designated as the medical authority to be recognized by the State. As the State has not a recognized medical authority, and as the degree at the colleges of the so-called sects, according to the authority of the medical profession of the world, is being conferred upon a false basis and upon those who are known to the profession to be educationally incompetent, the inquiry is suggested as to what measures have been instituted by the profession as a means of counteracting or correcting these abuses, or of improving present educational methods.

Before proceeding with the inquiry, however, the method to be proposed in this paper may be presented at this time.

The method contemplated, it is thought, may not only afford the means of correcting the educational abuses referred to, but may also serve to secure a decision by the court, as to the medical authority to be recognized by the State, without which educationally competent physicians cannot be secured.

Colleges are chartered with power to confer degrees, and the abuse and misuse of the franchise which must be corrected

consist in conferring degrees under the powers of the charter with an alleged educational requirement upon a basis which, by the medical profession of the world, is declared to have no value.

The method to be suggested in this paper is by the legal process of injunction, commanding the authorities of the college to show cause why they should not be required to adopt an educational requirement for a degree in conformity, therapeutically, with the authority of the medical profession of the world, and without regard to an alleged universal therapeutic formula.

With the reasons for the application, as suggested in this paper, there is not a court in the Commonwealth, not even excepting our Orphans' Court, with the President Judge on the bench, which would, or in fact which could, refuse the application.

No knowledge of medicine is required to enable the court to understand the conditions, and with the reasons presented, as here outlined, an injunction could not be denied, and the college could be compelled to adopt an educational requirement for the degree such as is required for the competent physician.

This plan is simple, is feasible, and would effectually eliminate imposture in the form of medical sects.

We will now revert to some of the means heretofore suggested as corrective of the conditions under consideration, and in order to judge of the trend of the profession in relation to these abuses which have so long existed we may refer to what has been said or written by those who have placed themselves on record, and whose position and influence in the profession have been such as to give direction to what has been attempted.

In an address on Higher Medical Education delivered before the Medical Department of the University of Pennsylvania, in 1877, Professor William Pepper said, in relation to the matter now being considered: "When all the difficulties of the question are taken into account, it does not seem possible that any plan can be successful which does not involve the intervention of governmental aid.

"There would seem to be no insuperable difficulty in framing a law which should prescribe the number of years to be devoted to medical studies before graduation, and which should further provide for the creation of a State Board of Examiners, who alone should have the right to confer licenses to practise within the limits of the Commonwealth.

"By this simple means all medical schools, regular, homeopathic or eclectic, would be compelled to provide such course of instruction as experience and sound reasoning approve, and the qualifications of all applicants for the license to practise would be passed upon by an impartial board, having no direct personal interest in the result."

Since this plan was suggested we have had some experience, and have gained some knowledge, as to what could be accomplished by the method proposed by Professor Pepper.

The impartial examining board is simply an impracticable measure, for the reason that the incompetents who receive their degree from a college in which a therapeutic formula is made the basis of the educational requirement could not successfully pass an examination before such a board, and would all be refused a license. To create a board by which they would necessarily all be refused a license after they had complied with the educational requirements of the college and received a degree, would be commencing reform at the wrong end.

The creation of a board of examiners was not to be expected, and was a measure that should not have been applied for. The defect is in the educational requirement for the degree and cannot, or at least should not, be corrected by a board to examine and refuse a license to all those who have received the degree.

In predicting that by the simple means of an impartial examining board all medical schools would be compelled to provide such a course of instruction as experience and sound reasoning approve, it must have been supposed that the rejection of all applicants for a license from the colleges of the so-called sects would follow, a method so indirect that it must be admitted to be impracticable.

Professor Pepper, however, accepts a modification of his plan, and writes as follows:

"I am entirely in accord with the views which my colleague, Professor H. C. Wood has expressed on this vitally important subject (*Lippincott's Magazine*, December, 1875, page 710), and I feel with him that the existence of the so-called medical sects offers difficulties in carrying out this plan which are much more imaginary than real."

He then quotes from Professor Wood's paper as follows: "To a board which examines in all such branches of medical science as are common to all the sects, might be appointed supplemental boards, which should examine in therapeutics only, the candidate selecting at will the representatives of the regular, homeopathic, or eclectic system."

"This plan," says Professor Pepper, "would apparently meet all the requirements of the position. It would not interfere with the medical colleges which would still confer the degree of M.D. precisely on such terms as they chose to do."

I need quote no further to show that these gentlemen in their suggestions do not seem to be in affiliation with the medical profession of the world. Medical sects are so called because they adopt an alleged therapeutic formula, and, while the medical profession of the world is authority for the assertion that a universal therapeutic formula has not been scientifically established, and does not truthfully exist, we find Professor Wood recommending supplemental boards in order that the incompetents who hold a degree of the sects may receive a license to practise.

It would be interesting to receive from Prof. Wood the formula of the therapeutic system which he designates *regular*. He seems to place the system by that name among the sects; yet, whether the formula be named regular or irregular, similar or dissimilar, eclectic or epileptic, faith-cure or sure-cure, the *regular* is not a sect unless it has a therapeutic formula. As we have no use for the term *regular* until we have a therapeutic formula to which to apply it, the term should be discarded. As to the present law by which three boards of examiners are created, one of these being to examine those who are qualified to appear for examination before a competent board and the other two adapted to the standard of the incompetents in order that they also may receive a license to practise, time will not permit of the consideration that the demerits of the measure demand, and unless the members of the board which is to examine the competents, can be made ashamed, and should refuse, to act in the capacity of taking twenty-five dollars from the pockets of the competents, in order to place them as physicians before the public on a plane with the incompetents, it is to be hoped that the law may be soon repealed. Co-incident with, or previous to, a repeal of this law, an injunction should be applied for in order that the authorities of any college within the limits of this Commonwealth under the control of a so-called therapeutic sect should be commanded to show cause why it should not be required to adopt a standard of educational requirement for the degree of doctor of medicine without regard to any alleged universal therapeutic formula.

This plan would apparently meet the requirements better than that of Prof. Wood, which was, not to interfere with the colleges, but was to permit them to "still confer the degree of M.D. precisely on such terms, as they chose to do."

The Philadelphia County Medical Society, being the only county society in the Commonwealth which is quoted as having endorsed the bill that became the present law to create three boards of medical examiners, besides being the largest county society in the State and the representative of the profession of Philadelphia, seems to be placed in a position of having considerable responsibility in the matter and is the one society in this Commonwealth to which the profession and the public should look for protection of their interests.

THE PROTEAN INFLUENCE OF PREGNANCY ON IDIOPATHIC EPILEPSY.

F. SAVARY PEARCE, M.D.

[Read December 23, 1896.]

The search for accurate clinical records of epilepsy in its relation to and modification by pregnancy and the puerperal state, with the remote effects of childbearing upon the mother's psychosis, is a difficult matter. For various reasons, especially the desire for maternity, the epileptic mother withholds imparting her true condition, and the severity, nature and frequency of her attacks. So likewise do the parents and relatives of the patient, and when a child is born, it too will they stealthily shield from any existing stigmata, as a mental enfeeblement born of its allied progenitor. I have had occasion to note this in another place.†

Similar obstacles are again encountered when an endeavor is made to trace the cause of the epilepsy and from this inexact knowledge many cases are still classed as idiopathic epilepsy, which, therefore, remains legitimate, yet, perhaps, inexpedient scientific nomenclature, but we are not justified in changing the name idiopathic until a cause *is* found.

Pregnancy may, it seems, ultimately produce epilepsy; *first*, by causing puerperal convulsions, and *second*, by irritation or exhaustion. In many cases, when a frank history is obtained, the etiologic factor is still not at all clear and we have to fall back upon the neurotic heredity and temperament, and these must only imply irritable neurons. In trauma, the specific infections and reflex irritations the cause is often detected. Conscientious fright appeared in one of my cases as a cause of epilepsy. Withal, many doubtful cases exist (the majority) which from lack of knowledge must be called essential. I propose to deal with these cases alone.

As to the explanation of the attack, whatever the cause, in contradiction of Golgi's theory of anastomosis of neurons as Leyden jars, we propose the theory of the motility of the neurons, or the discharge-theory suggested by Bechterew‡, Forel, His, and Cajal, abroad, and by Dercum§ in this country, which can be applied with nice precision to the motor explosions occurring in this most singular disease; for such irregular contractions of the protoplasmic mass may separate the dendrites to such an extent as to cause great incoördination of molecular force ordinarily held in abeyance by their normal coaptation. It seems just to infer that the cell-degenerations that sooner or later occur in cases of idiopathic epilepsy, follow the bad habit of the cell referred to, and, of course, its nutrition being interfered with, structural change must follow. As yet no accurate dividing line can be determined between the functional perversion and the organic cases, although in this disease, as in all affections, the pathologic histologist is placing morbid states one by one in the latter category.

The literature of the effect of pregnancy on epilepsy is meager. In Hare's

† Transactions American Academy of Medicine, 1895.

‡ Neurolog. Centralbl., 1896, Nos. 2 and 3, "Doctrine of the Neurons and the Discharge-Theory."

§ Functions of the Neuron, *Journal of Nervous and Mental Disease*, August, 1896.

"Essay on Epilepsy," the subject is traced down to 1889. This author says: "The influence of pregnancy on epilepsy is another point of interest and dispute, some claiming that the fits are increased, while others say they are diminished in frequency." During *labor* a fit is extremely rare in the epileptic. Tyler Smith,|| has seen only two attacks occur in fifty-three deliveries of fifteen epileptic women. The converse of this is also exceptional, but we know of a case in which the babe was born in convulsion.

Several unusual cases coming under my personal observation relative to the subject under consideration, presenting different phases of pregnancy and epilepsy, form the nucleus of this study, inasmuch as the anamnesis is complete and the progress of the cases has been traced up to date.

Case I.—Mrs. J. J. G., twenty-three years old, was seen with Dr. W. E. Hall, of Burlington, N. J., in September, 1896. There is no trace whatever of epilepsy or other neurotic disease in the ancestry. Without known cause and while in otherwise perfect health attacks of *petit mal* began at thirteen years of age. They came at irregular times, were very slight and momentary. Anything she was holding at the time would drop from her hand. She never fell, but simply stopped at whatever she was doing, and went on directly afterward. There was no aura. Menstruation was established at fourteen years, normally, and had no influence whatever on the spells. The patient was married about a year ago (at the age of twenty-two), concealing her trouble from her husband. The attacks went on "the same, or now even more frequently." She promptly became pregnant and the attacks continued irregularly once or twice a month the same as before marriage. An apparently healthy, even robust, girl, weighing five pounds, was born at the end of nine months. Labor was normal. The mother is a small woman and now presents the typical epileptic facies. No other disease can be found. She says her milk came very freely for the babe and that she at once became greatly "run down." At the third month of her puerperium she had her first and violent attack of *grand mal*. This was three months ago, September 27, 1896. Similar attacks have recurred once in from ten days to two weeks in spite of weaning the child, and of a tonic line of treatment during four months up to the last time I heard of her, November 19, 1896. The child is apparently normal.

This case of idiopathic epilepsy then developed *petit mal* at thirteen years of age. The patient was married nine years later. The attacks increased in frequency during pregnancy; a healthy (?) child was born, which was nursed thirteen months. The mother then being run down developed *grand mal*, which has become more manifest slowly up to the present time.

The following two cases occurred in the service of Dr. Alfred Stengel at the Howard Hospital, where I had the opportunity of studying them:

Case II.—Mrs. M. C., white, forty-two years old, was admitted to the ward September 4, 1896, suffering from chronic mucous entero-colitis and grave secondary anemia. Here, too, there is a negative neurotic history. The father had died of rheumatism; and this disease at fifteen years of age, and scarlet fever later were the only serious ailments the woman had suffered from. Nine months after the birth of her second child, at twenty-three years of age, and being much run down, she was seized while asleep with an "epileptic fit" and had several attacks that week. She, therefore, weaned the child. The attacks were preceded momentarily by an aura consisting of a "peculiar" feeling in both her thumbs. She bit her tongue in most of the attacks, which were characterized by general clonic convulsions followed by tonic spasm. She knew nothing for some minutes after the aura; and was somnolent an hour or two after a fit. From no apparent cause three years ago, in her thirty-ninth year, the attacks, which had occurred from every four to six weeks, gradually lessened in frequency, and during that year finally ceased *abso-*

|| *Lancet, XXIV, p. 644, 1849.*

lutely. The patient had had a third child two and a half years after the birth of the second. There was no lessening effect on the mother's attacks during this pregnancy. She has had no other children. The third child she affirms to have been normal. It died of diphtheria at six years of age.

The anemia lessened *pari passu* with the cure of the enteritis, the blood-count going up from 1,600,000 red blood-corpuscles and 16 per cent. of hemoglobin to 2,400,000 corpuscles and 30 per cent. of hemoglobin during the five weeks she remained in the hospital.

In this case it seems probable that the debilitated condition of the patient consequent upon child-bearing was the predisposing factor for the development of epilepsy. The attacks were severe, lasted sixteen years, then finally stopped and during that time a third pregnancy did not influence materially the fits. The free interval of three years, however, is not positive evidence that the attacks will not return later, although her recent severe anemia, it would seem, might have brought them on if the epileptic state was still latent.

Case III—H. B., a colored woman aged twenty-four years, gave a negative family history. She had always been "nervous" and had a bad temper. Two years before I saw her (in August 1895, when she was pregnant four months with her second child), the patient says she had her first convulsion. This followed six weeks after the death (from marasmus) of her first child at the age of nine months. The mother's vitality had been reduced in caring for, and nursing, the infant. She gave a history of having had irregular attacks of *petit mal* several years before, not related to the menstrual epoch, but somewhat increased in frequency after her marriage. Still later, during her first pregnancy, the spells became less frequent, and she had no attacks of *petit mal* during gestation until the sixth month. They continued after this every five or six weeks until the development of the major attack as noted, in which she was unconscious, bit her tongue on both sides and remained stupid for some hours afterward. The major attacks had recurred then pretty regularly every four or five weeks up to and including this second impregnation. Urinary examination was negative. Other organs were healthy.

This woman was delivered on January 29, 1896, at term, of an apparently healthy child, which, aside from a persistent facial eczema, is in good condition and nursing from its mother. Since delivery the patient has had attacks of *grand mal* on March 25th and 26th, a slighter attack on April 6th; again very severe attacks on July 9th and 20th, August 20th and 28th. She then skipped September and October to November 23d, when she had a violent explosion, the premonitory symptom of which was frontal headache from 6 to 11 A.M. Ringing in the ears while making a bed was the only aura. She screamed and remembered lying down in bed, under which she was found in an unconscious condition a few seconds later. She still persists in nursing her baby at the tenth month. The latter has not cut any teeth.

This case of *idiopathic petit mal* beginning at about seventeen years of age, was made worse (or was it a coincidence) after marriage at twenty. The spells stopped entirely during the first six months of her first pregnancy, again increased, and finally at the tenth month after labor she developed *grand mal*; which continued on and through a second gestation without notable amelioration.

These three cases illustrate varied modifications of the epileptic state by pregnancy. In order to more definitely investigate the subject I have made further inquiry with the following results:

Dr. Barton Cooke Hirst has sent me notes of the following three cases in his practice:

Cases IV and V.—"The two patients of mine who had epilepsy before confinement did not give a very distinctive history afterward. In one, who had usually but a single attack a year, there has been no return whatever of the trouble, since her confinement some eighteen months ago. The other case in which there had been no attack for a good many months prior to pregnancy, broke

out again quite badly in two successive convulsions, the first when her baby was about three or four months old; but under careful treatment since, including bromid, there has been a period of about three or four months free from convulsions."

Case VI.—"In looking over my notes I find that I have still another case of epilepsy in pregnancy. This woman had her first attack between twelve and thirteen before puberty was established. Following the first period the fits recurred always just before or just after the menstrual flow, but not every month, from six to nine during the year. If the convulsions, however, did not occur, choreic movements of a violent character regularly appeared during this period. This was her history until she married and became pregnant. During pregnancy there was a convulsion in the first and third months. There was not another convulsion during the next fifteen months, that is, until the baby was weaned and menstruation had returned. Then they came back just as before and she has had them ever since. Three months ago, November, 1895, they came somewhat more frequently, and for the first time in her life between the periods.

"From the close association of the convulsions with her periods I was induced to remove the ovaries, but so far the operation has not had a very gratifying result; she is just about as bad a year after the operation as she was before."

In case V it would seem that the physiologic phenomena of pregnancy took up the nervous force in this woman and prevented the attacks during gestation and subinvolution. How much longer cessation of fits (now one and one-half years into the post-gravid state) will continue is the anxious question.

This instability of nerve-center coördination is illustrated in Case VI, in which three months after pregnancy the attacks recurred severely after a lapse of many months before the gestation.

Case VII is very instructive, as the patient was carefully treated medicinally from childhood without avail. The fact that the epilepsy began before puberty, without other disease, would make it an idiopathic case. The intimate relation of the attacks later to the time before or after her menstrual periods, at times replaced by choreic movements; the great lessening of the fits during the pregnancy, only to again be lit up fifteen months after labor when the periods had returned; and the final just ablation of the ovaries, all prove that the genital organs were only irritants, not causes, of the fits, which have continued.

This case illustrates in part the statement which Dr. S. Weir Mitchell was good enough to make for me:

"I have had considerable experience, covering many years in this matter, and I can say that I have never seen epilepsy made worse by pregnancy; I have frequently seen it made better—the attacks occurring less often. This seems to me to be accounted for by the absence of menstruation, which is a condition in epileptic women inducing attacks; at least the spells at that time are more frequent. I have seen cases (I cannot remember how many) in which the attacks have entirely ceased during pregnancy; in one of such cases the spells did not return until after seven months of lactation. I do not know that the presence of the ordinary type of epilepsy without known organic lesion has any effect upon the dangers of confinement."

Cases VII, VIII and IX.—Dr. Richard C. Norris sends the following notes of his experience at the Preston Retreat: "There have been three epileptics in the last 750 cases. In two of the cases the patients had their fits more frequently throughout pregnancy; in one case the patient said she felt better and the intervals between attacks were longer. I have had no opportunity to learn the subsequent histories of these cases. The first two cases mentioned were multiparæ, the third was a primigravida."

Case X.—In August, 1896, a colored woman, thirty-three years old, was brought in the "patrol-wagon" to St. Agnes' Hospital, having fallen on the street in a general epileptic convulsion. She gave a history of idiopathic epilepsy from the age of fourteen, had borne three children and had never had an attack during pregnancy. The patient told the interne, Dr. Milligan, that she

"wished she could be with a child all the time."

Drs. R. P. Hamill and W. C. Goodell permitted me to look up the records at the Maternity Hospital, but I could find no epileptic cases among the 300 women delivered in that institution.

I have gone over all the cases at the Lying-in-Charity Hospital with the kind permission of Dr. W. Reynolds Wilson and Dr. George M. Boyd. Of 1,200 consecutive cases the records are very complete in 482. While there were the usual number of cases of uremic convulsions during and after pregnancy, I could find no distinct case of epilepsy during gestation, and Dr. Wilson writes that he has no knowledge of any such cases that may have been omitted in note-taking. As women are only in maternity hospitals several months, attacks may well have not occurred during that time; hence such meager data might be misleading.

At the "Midnight Mission," where girls illegitimately pregnant are kept from the first up to the eighth month of gestation, the matron tells me that out of 152 cases in the last two years only one was subject to fits. The relation to the pregnancy was not ascertainable.

To prove that attacks of epilepsy during pregnancy are difficult to trace I have made inquiry among general practitioners, with the result that few men of wide experience have any records of such; and who would have better opportunity to follow out cases than the family physician?

Dr. H. H. Herbst of Allentown, who has the record of but one case of idiopathic epilepsy and pregnancy writes as follows: "I have a case (XI) under observation at present in a woman twenty-seven years old, whose first child is now six months old, and so far she has had only one fit since the birth of her child. Before her pregnancy she had attacks once a month lasting from ten to twenty minutes at a time, this even under the influence of large doses of bromids. During the whole period of gestation she only had one fit, and this was of short duration, and the bromids had been reduced one-half during pregnancy. She had no bromids during the puerperium, until ten days ago, after her first fit."

Dr. Morris J. Lewis makes this general statement in regard to two cases of pregnancy in idiopathic epilepsy. "In both cases, aged twenty-five and twenty-nine respectively, there was no attack during pregnancy, only to be recurrent afterward."

Dr. Chapin informs me that in the 6,800 women admitted to the Pennsylvania Hospital for the Insane he has no knowledge of pregnancy having influenced the epilepsy. Of course epilepsy with maniacal outbursts only would be admitted to a hospital for the insane, and therefore these figures have but little bearing upon the subject under consideration.

In recently going over the books at the Infirmary for Nervous Diseases for another purpose, I found since 1889 some 300 cases of epilepsy recorded, of which seventy-seven were in females at maturity, married or single, of whom seven bore children or at least became pregnant, and the histories are traceable.

Case XII.—E. D., thirty years old, has had epileptic seizures since the age of twenty-eight years. The supposed cause was a miscarriage in February 1890, after which she was very nervous. Three months later at night she had the first attack, bit her tongue, etc. The second attack occurred three months after the first, the third nine months later, when she had also several seizures of *petit mal*. At further intervals of seven weeks and two months she had her fourth and fifth attacks respectively. Dr. de Schweinitz found the optic discs too red and also slight astigmatism.

This case of epilepsy seems to have been excited by a miscarriage. The patient has had no children since.

Case XIII.—M. B., a white woman, aged forty-four years, presented no psychotic or neurotic heredity. She menstruated first at fifteen years, and normally until the age of thirty-four years, when the periods became irregular. She was the subject of amenorrhea for four months. At this time she became very nervous and one night she had a regular epileptic attack, frothed at the mouth, and had a clonic convulsion. The tongue

was not bitten. She had borne two healthy children up to the time of her illness, but none since. When she reported to the clinic on March 6, 1896, she was anemic and troubled again with amenorrhea for the preceding six months.

This woman was clearly perturbed by the monthly irregularity. There has been no special relation of the attacks to the menstrual periods however. The menstrual disorder was no doubt the exciting cause of the epilepsy.

Case XIV.—M. B., a white woman, twenty-nine years old, is not an example of idiopathic epilepsy, but her case is here mentioned only to show how pregnancy may be the cause of epilepsy through an apoplectic attack. It leads to the thought too: Why should not puerperal convulsions be more frequently followed by epilepsy?

This woman, three weeks after labor, at the age of twenty-six, had an apoplectic stroke, with consequent left hemiplegia and left lateral hemianopsia. Speech was thick for a month or so. There was no loss of sensation. Two weeks after the stroke she had a clonic convulsion confined to the left side. Since then she has had a fit every two weeks, beginning on the left side, but in the last year also becoming general.

Case XV.—A. V., a white woman forty-one years old, has three children, living and well. One child was said to have died of hydrophobia. The patient had menstruated first at eighteen years, at which time she was married, by the advice of her physician. After the first intercourse she had a convulsion, with general movements, bit her tongue, and was unconscious for four hours. She then had attacks every night until she became pregnant. The attacks now ceased until two months after delivery, when they recurred, though less frequently. *The woman has borne five children, and never had an attack while carrying them.* At the age of thirty-seven years she had both ovaries removed, but without effect on the attacks, which have continued about once in three weeks.

Evidently here is a woman of neuropathic diathesis, whose delayed menstruation was brought on after marriage at eighteen, and with a convulsive seizure at the same time. As the ovaries removed were normal we must call the case an idiopathic one. At the same time a reflex irritation, originating in the genital tract, appears to have excited the epilepsy. Perhaps the nerve-energy mainly diverted to the womb during gestation then stopped the attacks by reducing the afferent impulse-overflow to a minimum.

Case XVI.—E. B., a woman aged forty-two years, had no hereditary taint. Menstruation was established normally, and she was married at the age of twenty-one years. The attacks began at the age of twenty-five years. The patient has borne six children, all in good health excepting one that was treated at the clinic for chorea. The patient has never miscarried. The first attack occurred at night. The supposed cause was a sore throat. This woman reported in March, 1892, having on an average an attack every week. Dr. Hirst could find no abnormality of the genitalia. We can find little evidence of modification of the epilepsy during her rather frequent pregnancies, so that the case illustrates the fact that in rarer cases the seizures may go on practically uninfluenced in a prolific mother, and the children not seem to manifest any neurosis.

Case XVII.—A. B., twenty-two years old, menstruated first at the age of seventeen years. She had a vague history of four or five attacks of *petit mal* when seven years old, but none from then until the age of seventeen years. The first fit occurred on the third day of her first menstrual period. After this she had an attack every month. At the end of six months she was married, and a healthy child was born in nine months. The attacks were somewhat lessened during gestation. With the puerperium the attacks began to occur weekly, and for six months previous to the date of reporting she had, besides, a reversion to slight attacks (*petit mal*), of which, at times, five occurred in a day.

This woman was evidently drained by the pregnancy, and especially during lactation, so much so as to further reduce her, and thus, very probably, to increase the attacks.

Case XVIII.—F. R., forty years old, reported January 16, 1895. Her menstruation appeared only after marriage, between seventeen and eighteen. Here again marriage and coition set up epileptic seizures apparently, for the woman had never had a fit before. She has borne three children. One is said to have died of hydrophobia; one died while teething, and one is living and well. The patient never had a miscarriage.

She has had no attacks during pregnancy and for some months afterward, when they again recurred. Menstruation has had no effect on the attacks in the interim. Oöphorectomy six years ago seems to have lessened the frequency, but not the severity, of the fits. The urine and organs were normal. At present she has an attack once or twice a week.

The case rather illustrates again the intimate relation between the sexual organs and the higher nerve-centers, brought out prominently in certain individuals, and yet in the absence of evident disease. It is only a different phase of a neuropathy, as exemplified in the delusions, hallucinations and morbidity of the sexual neurasthenic. The difference is only an expression of the excited neuron.

Case XIX.—Dr. Helen Kirschbaum has seen but one case of pregnancy and epilepsy at the Jewish Maternity, and that is an interesting one. A German woman, aged twenty-five years, a primipara, reported in December, 1893, giving a history of generalized attacks of *grand mal* for several years, occurring once in three months, and mostly at night during sleep. She would be stuporous for several hours after the fit. After pregnancy this woman began at once to have her attacks at intervals of from one to ten weeks. She was brought into the hospital with the os dilated and a transverse presentation. It was thought that labor was about to come on. Version was effected and, to Dr. Kirschbaum's surprise, when the fetus was placed in its normal position labor was delayed, and the woman went to term and was delivered of a healthy child a month later, and remained under observation for two months into the puerperium. It is of interest to note that the attacks did not occur after version was performed, although they had taken place weekly immediately preceding. This fact seems rather a co-relation than a coincidence when it is remembered that the attacks were distinctly increased after pregnancy, and it leads to the thought that the gravid womb and the malposition of the fetus excited attacks. From the foregoing study the following

CONCLUSIONS

may be drawn:

I have thus detailed nineteen cases of pregnancy in idiopathically epileptic women. Where not otherwise mentioned the kidneys in all cases were normal. The evidence is pretty conclusive that there is a wide range of effect on the number and frequency of epileptic seizures in pregnant women the subject of this disease so little understood. The results of this research also show clearly that in the majority of cases the attacks are lessened in frequency during pregnancy. This would preclude the possibility of intestinal auto-intoxication being the primal cause of any epilepsy, for during gestation constipation is the rule, and toxins therefore are more readily generated in the gastro-intestinal tract. Yet it cannot be doubted that such poisoning is an excitant of the already unstable brain-cell in exceptional cases.

As stated in the body of the paper the referring of nerve-energy in the physiologic process of pregnancy to the uterine organs prevents any excitation of the motile brain-mass, and in those cases in which the attacks are increased some irritation must exist that we are not able to fathom. It is well known, too, that attacks are often lessened in inter-current disease in epileptics.*

Then there are two classes of epileptics, one in which degeneration of the neuron soon supervenes upon idiopathic epilepsy, and a second in which degeneration is much delayed after these terrible explosions. The great Napoleon was an example of the second group.

* Other references: (a) Brown-Sequard and Bourneville, Researches on Epilepsy, and Phys. and Path. Cent. N. System, 1857-1860; (b) Hubert, Des Convulsions, These de Paris, 1857; (c) Savary, Dict. Enc, 60 vol., Art. Convulsions, 1813, V. VI.

Which incipient cases in pregnancy belong to the first or to the second group is unknowable. Therefore the protean effects of the normal drain of pregnancies upon the epileptic woman cannot be foretold. We can but hope to cut off the marriage of epileptics as a means of positive favorable solution in favor of the present and future generations.

In conclusion I desire to thank the physicians who have kindly contributed to this collective study.

DISCUSSION.

Dr. L. J. Hammond said that he had seen four cases of epilepsy in 2,200 labors. In two of these cases there were no epileptic attacks during the pregnancy, but in both cases the attacks recurred during lactation with greater frequency, and in one with greater severity. In the third case the pregnancy seemed not to have affected the seizures in any way. In the fourth case, one of *petit mal*, the attacks were apparently not interfered with until after delivery, when they became much more frequent in occurrence. There was no evidence of epilepsy in two of the children whose subsequent course was followed.

Dr. H. A. Hare referred to a case of idiopathic epilepsy occurring about the time of puberty in which the woman was married with the hope that marriage and possible pregnancy might relieve her attacks. Marriage did not relieve her attacks, while pregnancy did very materially decrease their number; but after pregnancy the attacks came on with quite as much frequency and force as before; indeed, they increased in frequency. This case was typically idiopathic in history, and it was typical because no remedy did good. So little is known about the cause of idiopathic epilepsy, and the whole question as to reflex epilepsies is so clouded by contradictory statements of eminent neurologists, that conclusions cannot be drawn from the paper, which points distinctly to the fact that idiopathic epilepsy is temporarily influenced favorably by pregnancy.

Epilepsy is a hypothetic disease to everyone except to the individual who has it, and while statistical papers are of value, knowledge pointing to the cause of this extraordinary disease is yet wanting.

It may be that the studies made in the last few years by neurologists in regard to the neuron will yield some information in the future, but no neurologist has been bold enough to claim that these studies have done much to explain what idiopathic epilepsy means. Examinations of the cerebral cortex, except when well-marked gross lesions have appeared, in ordinary epilepsy, have shown no change that gives any idea of the pathology of the disease.

Dr. A. A. Eshner said that it is generally recognized that during pregnancy in an epileptic subject the attacks are sometimes diminished in both frequency and severity; so likewise are they influenced in intercurrent febrile disorders. Dr. Eshner was able to recall at least one case of epilepsy complicated by typhoid fever in which the convulsive attacks remained in abeyance throughout the entire acute illness, and he spoke of a similar observation in a case complicated by pneumonia. With the termination of the complicating disease the convulsive seizures recur, sometimes with increased severity and frequency.

Dr. Eshner expressed regret that the qualification "idiopathic" had been employed. Failure to discover a primary etiologic factor does not necessarily imply its absence. To employ the qualification is to assume knowledge we do not possess. As this increases such usage diminishes. In the absence of a demonstrable cause it would seem sufficient to designate the case simply as one of epilepsy. When such a cause is obvious there can be less objection to the use of such qualifications as "traumatic," "toxic," "reflex," etc.

Dr. F. Savary Pearce said that there is no doubt that many cases of imbecility result from the impregnation of epileptic women. On the other hand, notable exceptions occur, as in two cases known to Dr. Pearce personally. These children are remarkably well developed. In two of the cases reported in the paper read the children are apparently in perfect health, and present no stigmata of nervous disease. While physicians may concur in favoring the non-marriage of epileptics, they cannot always positively say to a family that any children born will be impaired mentally. There are cases born of epileptic mothers that are quite as normal as other children, although this is not the rule.

Dr. Pearce was disposed to insist upon retaining the term idiopathic epilepsy, especially in view of the fact that not much is known about the cause of the disorder. When a cause is known, such as trauma or brain-tumor, the appropriate qualification can be employed. In other cases the term idiopathic can be used with propriety. Idiopathic epilepsy is nothing more than a manifestation of irritability of the neuron as a result of which at times an epileptic seizure results. No degeneration of the nerve-cells is demonstrable, whereas in other cases (organic) degeneration takes place.

A REPORT OF CASES IN ABDOMINAL SURGERY, WITH REMARKS.

MORDECAI PRICE, M. D.

[Read December 23, 1896.]

Appendicitis. It is the accepted opinion of every surgeon throughout the civilized world that when the diagnosis of appendicitis is made the treatment is purely surgical. So says one, so say all. No one questions the correctness of this treatment for a moment. It is also easy to say when the appendicitis exists. The hardest part for both the physician and surgeon is to say when it does not exist.

I cannot accept the opinion that some men seem to hold, that all inflammatory troubles in the peritoneum, first, last and all the time, are appendicitis. It has been my experience constantly to meet with numbers of cases with troubles closely resembling appendicitis, with symptoms so closely resembling those of inflammation of the appendix, that it was impossible to say that appendicitis did not exist until free purgation had been used, when there was ample proof that other causes entirely separate from the appendix were at the bottom of the sickness. These conditions occur in my experience at least five times for every case of true appendicitis that I have been called upon to treat. Free purgation in these cases will clear up the symptoms and remove the trouble, and when it does not accomplish this most-to-be-desired object, it makes very plain the fact that we have to deal with true inflammatory conditions of the appendix, or an ulceration of the colon itself.

This confusion of the term "appendicitis" and other inflammatory conditions in the peritoneum has been the cause of the constant controversy between men on this subject, some accrediting cures of true appendicitis and abscess at the head of the colon to purgation and poultices. Simply because they had the symptoms of appendicitis—pain, tenderness, increased temperature, and a mass in the right iliac fossa—they concluded without hesitation that they had unquestionably a case of appendicitic abscess. Free purgation cured the case. They did not appreciate the condition present, as can be seen from the following report of a number of cases that completely represent just this condition, and in none of them could true appendicitis be positively excluded until free purgation was brought about, when the whole matter was cleared up.

Could men look at all the conditions and weigh them earnestly before laying claim to any particular line of treatment, there would be but one opinion in a very short time, both among physicians and surgeons, as to what should be done in these cases. There are undoubtedly recoveries from true appendicitic abscess, from rupture into the head of the colon, into the bowels attached to it in other places, or into the bladder, or externally, at the usual seat of operation. These are fortunate accidents in the termination of a most serious condition, and could not, under any circumstances, be considered rational results of treatment. The great improvement in this particular disease is more in the line of diagnosis, the general practitioner not being satisfied with

simply calling the condition peritonitis. I have found in my intercourse with the medical profession that its members know what appendicitis is, its gravity, its symptomatology and the treatment, and it is very rare indeed that I have the opportunity of making a diagnosis for them.

Symptomatic and True Appendicitis. I desire to discuss this question under the two headings from my own experience in this condition.

Symptomatic Appendicitis.—(I use this term because for a time it is impossible to separate this condition from true appendicitis.) The symptoms of this condition are from the very beginning those of true appendicitis. It may set in with a violent chill, high temperature and agonizing pain, following a history of constipation, after a large and gluttonous meal, with a large mass in the right iliac fossa. There may be a mass without elevation of temperature, or chill, but with severe colicky pains. To say that appendicitis does not exist under these circumstances is a very difficult matter. The appearance of the patient and the existence of a mass at the very outset of the disease is to me almost satisfactory proof that if appendicitis exists it is catarrhal in character and is caused by the damming back of its secretion by the collection at the head of the colon. The treatment of this condition is purely medical. George B. Wood early recognized just such a condition and also formulated the treatment—free purgation by calomel and salts. As to the frequency of this condition, I have probably seen as many as five cases, in consultation, to one of true appendicitis, and the treatment by purgation for the cure of this condition is as essential for good results in the treatment of the true form of the inflammatory condition of the appendix, which we all recognize to be a surgical disease from the beginning.

One of Philadelphia's most illustrious teachers, in discussing this subject of appendicitis, says that just in proportion as a mass makes its appearance in the right iliac fossa early in the condition is the prognosis favorable without surgical interference. This assertion I endorse, and I believe that all such cases are due to impaction, and are curable by free purgation. I have several cases that I desire to report to illustrate the position which I take on this subject.

Dr. Weaver asked me to see a boy with him suffering with appendicitis. He was called to the boy on Sunday afternoon and found him with a temperature of 103.5° and a pulse of 130, and in great pain, with the abdomen greatly distended. The following history was obtained: The boy, thirteen years old, and much overgrown for his age, had on the previous Saturday fallen on the railroad, suffering a slight injury in the right groin. This did not prevent his taking a long ride on horseback on Sunday morning. In the afternoon the doctor was called and found him, as stated, with a large and tender mass in the right iliac fossa, excessively tender to the touch. The boy lay with his limbs drawn up, and everything seemed to indicate a dangerous condition. I saw him on Monday morning. Dr. Weaver had purged him all night and had done all he could to remove the trouble. After careful investigation of the symptoms, I considered the case one of impaction caused by a slight blow in the groin and the boy's ravenous appetite on the previous Saturday. I endorsed the treatment and asked that it be continued and that I should be called if the boy was no better on Tuesday at noon. I received word on Tuesday to come at once, that the boy was dying. So soon as I could go, I went. Dr. Bricker accompanied me, and when we arrived we found the boy sitting on the side of the bed, with all the urgent symptoms gone, and when we asked for the cause of the great improvement, the chamber was produced and it was half full and more with partly digested vegetables and other things that boys will sometimes eat.

Why impaction instead of appendicitis? First, the dull character of the pain; then the high temperature, which is not usual with pus in the peritoneum, and the large mass in the right iliac fossa at the very beginning of the symptoms. The history of the blow in the region of the head of the colon was suf-

ficient cause for partial paralysis of the bowel and the distention.

The following two cases are reported by Dr. John F. Roderer, with whom I saw them in consultation:

Mr. M. retired on Sunday, October 25th, very well. He awoke about 2 A. M. with pain in the abdomen, which became very severe. When I first saw him he was suffering with severe colic and with pain in the right iliac fossa. The bowels had been moved on the previous day. On examination I found a large mass in the region of the head of the colon. There was considerable tenderness on pressure; and the temperature was but little above the normal. After free purgation all of the symptoms disappeared.

On the 5th of November I was called to see Mr. W., who had a severe attack of colic, with a great deal of distention and tenderness over the entire abdomen. The iliac region was distended and tender, with considerable induration. The bowels had been moved the previous evening. In the afternoon the temperature was 104°. During the following night the bowels were freely moved, and the distention and tenderness all passed away.

Grave appendicitis without grave symptoms. —"Mattie G., aged twenty years, had always enjoyed fair health. On July 19, 1896, she noticed a slight flatulent swelling of the abdomen, which continued until July 23d, when pain developed, which could not be located, but was described as a general abdominal soreness. Late in the day it was referred to as located above the umbilical region. The pain now became severe and of a character so continued as to cause the patient to pass a very uncomfortable night. I first saw her early Friday morning, when I learned the foregoing history.

On examination I found the abdomen tympanitic. There was almost general abdominal pain, with tenderness over the right colon, but no dulness on percussion. The bowels were slightly constipated, the tongue furred, with slight nausea and a temperature of 101° F. The patient presented a very sallow appearance. During Friday afternoon free purgation took place from salts. Before midnight the patient had a severe chill, followed by a second one at four o'clock on Saturday morning, after which the temperature rose to 105°. At seven it fell to 101°; at nine it had risen to 103°. The patient still was freely purged. The abdominal pain had almost disappeared; the local tenderness was not severe; the pulse was 120°. On percussion only slight dulness could be detected in the iliac region. At noon the symptoms were unchanged except that the temperature had fallen 0.5°. At this time the case was seen by Dr. Price, and as the rapid pulse and the pinched features indicated a more alarming state of affairs than could be accounted for by external examination, it was decided to explore by incision. This revealed an injected appearance of large and small intestine, with some adhesions. When the appendix was reached, it was found to be in a state of suppuration and so gangrenous that the mere elevation on the finger caused it to rupture. It contained about one teaspoonful of fetid pus, and a hardened mass of fecal matter the size of a date-seed at the base, which no doubt caused the constriction, thereby cutting off its circulation, with resultant gangrene. The appendix was tied at its base and removed, and the wound well irrigated and packed with gauze. On the following four days the temperature ranged between 102° and 103°. The discharge from the wound for the first forty-eight hours was dark, watery and very offensive; during the next forty-eight hours it was an ichorous pus, and then a free discharge of more healthy pus, with a fall of the temperature. From that time the patient made a continuous and uneventful recovery."

This report was made by the attending physician, Dr. George Romine, of Lambertville, N. J.

Dr. W. F. J., of Harrisonburg, Va., was seized with pain and uneasiness in the right iliac and lumbar regions, was chilly and felt so badly that he was not able to attend to his practice; but after free purgation he went about and on the fourth day called on some of his patients, but was glad to get back to his bed. A consultation was held by his neighboring physicians, who attended

him, Dr. Lincoln and Dr. Neff. In consultation with Dr. George Harrison, of New York, it was decided after a careful examination that the patient was suffering from appendicitis, that the abscess was walled off from the general peritoneal cavity, and that an operation was a necessity to save life. On September 11th I was telegraphed for, but could not leave until night, reaching the patient the next day, the interim affording plenty of time for his condition to become serious. He was suffering from great pain and constant vomiting, with a very distinct mass in the right groin, which was constantly enlarging, very tender to the touch and extending almost to the middle line of the body. We decided to operate at once. The patient was a very large man and quite fleshy. Having had his bowels well moved on the morning of the operation, we decided to do only an operation for drainage and not to molest the appendix unless it was positively demanded. After opening the abdomen and washing out from one to two ounces of pus, we did not think it good surgery to do anything with the inflammatory mass except to drain the parts with rubber and gauze; and this was done.

Before the operation the patient had been vomiting a black and ugly mixture of mucus and bile. This continued, and during the night it seemed to increase, and he became very restless and hard to manage. At midnight I was called to his bedside, and he demanded morphin. I decided to stay with him and administer small doses of calomel and soda every fifteen minutes. Toward morning an injection of half an ounce of Epsom salts in half a pint of warm water was given, and retained as long as possible. At five in the morning, without any movement of the bowels, but with considerable rumbling, showing that the medicine was having its effect, the patient had about half an hour's sleep. At eight in the morning his bowels moved freely and continued to do so all day, and from that moment the vomitus improved in color and quantity and the pain became bearable; all of the symptoms improved and the patient went on to a rapid recovery.

Operation for Appendicitis.—A.P., twenty-eight years old, a patient of Dr. Buzby, of Swedesboro, N. J., with a fine physique, had suffered for years from a very severe form of colic referred to the epigastric region. He did not remember to have had pain in the right iliac region during or between any of these attacks. On Thursday, September 11th, he had what he supposed to be one of his old attacks, with pain in the same region as previously located, and about as severe in character, with the additional symptom of sick stomach. The bowels were slightly confined. Dr. Buzby saw him on Friday morning and purged him freely during the day. On Saturday he had a severe chill. Free purgation relieved the epigastric pain and the distention of the bowels. At this time the patient began to complain of distress, in a few hours amounting to severe pain, in the region of the appendix. His condition pointing to appendicitis, Dr. Buzby telegraphed for me to come early on Monday morning and operate. When I arrived the only symptoms I found pointing to the appendix as the cause were the severe pain, the anxious appearance of the patient, a normal temperature and the fact that free purgation had not relieved the patient. I recommended operation, because to wait longer would have been to incur risk of life. The operation was a simple one. The bowel was injected; the appendix, buried under the head of the colon, and of almost bony hardness, was about as large as my little finger and presented an ulcer at the end through its entire thickness. Had it not been adherent and encapsulated under the head of the colon, the man undoubtedly would have been in a very dangerous condition.

Surgery of the Gall-bladder and Gall-ducts.—Surgery of the gall-bladder and gall-ducts has given in the near past most gratifying results, but must be oft-repeated in order that the entire profession may know that these cases can be relieved and that too with but little risk to life. Delay until the entire blood-supply and all the tissues of the body are so changed by the absorption of the bile-products causes the patient to be poisoned beyond help. This is

the one condition to-day that makes the report of the surgeon and the claim that all these cases should get well seem inconsistent. Operative procedures on the gall-ducts and the gall-bladder not only differ in character and magnitude and danger in the individual case, but they differ in the patients themselves. One man's case may be just so well managed as another's so far as the surgery is concerned, but the result must be in doubt on account of delay and inability on the part of the patient to endure any operation. In my work on the gall-bladder and gall-ducts, I am firmly convinced that the operation is a safe one if done early and with sufficient surgical knowledge to protect the surrounding viscera at the time of the operation, and leave them so protected when the operation is finished. This can easily be done by a careful operator. There are cases in which the stone will be very hard to deal with and in a position where we cannot close the opening in the duct by suture. Then we will have to place gauze so as to insure drainage and also protect the surrounding viscera from leakage of bile. This is not hard to do if one knows sufficient about abdominal surgery to use gauze properly for drainage and how to treat it after its day of usefulness is past and it must be removed. To know how to do this is to serve a long and careful apprenticeship at the operating table by the side of one who has done the operation many times and then watch the cases until all the dressings have been removed and renewed many times, for no operator treats all his cases alike; nor could they get well if he did. He has to consider the patient's condition, the character of the drainage, the danger of infection and leakage in every case, and the position of the opening in the gall-duct. If this last is low down, there will have to be one large drain of gauze and then many smaller ones, so that in the after-treatment of the case the smaller ones can be removed and the wound examined and the large one protecting the peritoneal cavity left so long as the safety of the patient will permit and by degrees the gauze and drainage can be removed. Nature protects the peritoneum by walling off behind the gauze, and as the gauze is removed, the drainage-track closes in and recovery is complete.

Two Cases of Gall-stones.—Mrs. G., of Wenonah, N. J., a patient of Dr. H. Stout, some twenty years ago suffered with agonizing pain for a short time in the region of the gall-bladder. From that time on to the middle of July, 1896, she averaged good health. She was over sixty years of age, a fleshy, thick-set woman of low stature, and active habits. She was seized with pain, and became slightly jaundiced. There was considerable tenderness in the region of the liver, with a well-marked mass of tumor, irregular in outline, immovable and nodular. Notwithstanding the fact that the doctor had used all means to relieve such a condition and to improve her, nothing did her any good. She steadily grew worse, and the discharge from the bowels was almost colorless. The jaundice deepened until her color resembled almost that of mahogany. From the outset her attendant insisted that it was gall-stones from which she was suffering, and begged her to submit to an operation for their removal. I was asked to see her in consultation, when the question of operation was discussed and urged by her attendant. Owing to the deeply jaundiced condition and the great prostration of the patient, the nodular, hard and immovable condition and the form of the tumor, and the additional fact that she was a large fleshy woman, I thought her to be suffering not only from gall-stones, but also from a rapidly developing malignant growth involving the gall-bladder, liver and stomach, as she also suffered from persistent vomiting, though this symptom is often present with gall-stones. She also suffered from constant pain in her right elbow and arm and shoulder, which disappeared after the stones were removed. I considered the case a very unfavorable one for operation, but could not move the doctor from his belief in gall-stones alone.

An operation was finally decided upon and performed on September 13th. As soon as the abdomen was opened a distended and sacculated condition of the gall-bladder was found immediately un-

der the incision. The gall-bladder was brought up into the wound and then carefully surrounded with gauze sponges to protect the peritoneal cavity. The gall-bladder was opened and about eight or ten ounces of pus and thickened bile removed. I then passed my finger and removed the top stone, which was about an inch in one direction and nearly equally thick, with one face smoothly polished. The other stone was about one inch in thickness and two inches in length, diminishing to a bulbous thick point that was securely wedged in the bile-duct, directly over the abdominal aorta, and was dislodged with difficulty from its bed and removed. The gall-bladder was evacuated of all its contents and then securely stitched to the peritoneal and abdominal walls. The condition of the patient improved from the outset. After the operation her temperature was 103°, and by the third day it had reached normal. On the evening of the fourth day the patient perished from hemorrhage from rupture of the abdominal aorta. The post-mortem examination was made by her attendant, Dr. Stout, soon after death. What the result would have been had she taken her attendant's advice some five weeks before to have the operation performed then is uncertain, but I am inclined to think that she would have recovered.

I have had six cases of gall-stones and they all recovered when they were not complicated by malignant disease, as in one case in which the gall-bladder contained a large stone as big as my fist and was covered with malignant fungoid granulations and ruptured. This stone I did not remove. The patient died in a few days.

Mrs. M. J., of New Hope, Pa., a patient of Dr. Clawson, suffered from gall-stone. For many years she had suffered from paroxysmal attacks of great severity, requiring large doses of morphin to relieve her. She was incapacitated for her household duties, was confined to bed for weeks at a time and was slowly drifting into the morphin-habit. During the operation, which was performed in May, 1896, it was found that the gall-bladder was not distended, but was displaced far back under the liver and surrounded by dense adhesions. These had to be broken up, the gall-bladder exposed and opened. Some ten or twelve stones were removed, with the gall-bladder, at least two inches from the abdominal wound. Thorough cleansing of the parts was effected, with suture of the gall-bladder with fine silk and a No. 5 cambric needle, and with gauze drainage to protect the peritoneum from possible leakage of bile. This patient made a rapid and excellent recovery, and since the operation has gained some thirty pounds in flesh.

DISCUSSION.

Dr. L. J. Hammond asked whether the cases referred to as fecal impaction, in which a great deal of pain in the right iliac fossa existed, were not instances of chronic appendicitis. It seems hardly likely that fecal impaction would localize itself in the manner described without an arrest of the function of the bowel at this point. In Dr. Hammond's experience the pain observed in cases of acute appendicitis is not relieved by free purgation, but is greatly increased, especially if pus has formed. In two cases of acute appendicitis in which salines were given it was necessary to operate immediately, as the pain was so greatly increased and the temperature went up so rapidly.

Dr. W. N. Woods related the case of a patient with an old hernia who was reported to be "suffering with extreme pain and swelling of the abdomen." The hernia had been down about an hour, and there was a great deal of pain in the abdomen and in the patient's wriggling it had come down and become strangulated. Manipulation soon reduced the mass, but the pain did not decrease. More careful examination discovered a very large hard mass in the iliac fossa. The temperature was about 102°. At once the administration of calomel and salts was begun. The bowels had not acted at midnight and in the morning the temperature had risen to 103°. With a consultant it was concluded that the patient had appendicitis and operation was advised. The man positively refused an operation, saying that he would die rather than submit to it. Attempts

to purge him were continued, with no material results until the evening of the fourth day, when the temperature had run up to 105°, without amelioration of the symptoms. The man was getting very weak and was expected to die. On the evening of the fourth day a high enema of castor-oil was administered (somewhere from a pint to a quart) and the patient advised to retain it if possible. About midnight the symptoms were entirely relieved. There had been a free purgation and something over a pint of cherry-stones had been passed. The man had previously been asked in regard to his diet, and he gave no history of excessive indulgence, or having eaten anything out of the ordinary run. After passing the cherry-stones he stated that he had been in the country the day before he fell ill and had eaten an immense amount of cherries and swallowed the stones.

Dr. W. S. Stewart said that almost every form of inflammation in any part of the abdomen is at present pronounced appendicitis. In his early experience, almost before appendicitis was recognized as a disease at all, all cases of that kind were treated with heroic doses of purgatives, commencing with calomel and following with castor-oil and turpentine. Many cases recovered, but it does not follow that the patient will get well because his bowels are kept open. That is not the ultimatum of the remedy in cases of impaction. The purgation must be kept up for days and weeks sometimes. Dr. Stewart referred to a case in which a tablespoonful of castor-oil and a teaspoonful of turpentine were administered every three hours, until forty ounces of castor-oil had been given. This case, a very serious one, made a perfect recovery. The patient is still living, after a lapse of twenty-five or thirty years, perfectly well, and has had no attack since. Dr. Stewart passed through a similar attack himself, with the same treatment, as that seemed to be the method in those days of treating all cases of impaction.

It is very important in the cases to be operated upon to have good drainage and good nursing. The drainage must not be stopped too soon, so that every particle of pus is drained out and the cavity kept perfectly clean.

Dr. M. Price said that the presence of pus in the peritoneum does not justify any surgeon in waiting after the diagnosis has been made.

As to the differential diagnosis between true appendicitis and other bowel-troubles, collections in the bowels, impactions and otherwise, Dr. Price has made it a rule always if there is induration, thickening and a mass in the right iliac fossa in the beginning of the attack, to say without hesitation that the case is not one of appendicitis. If the case is seen within twelve or fifteen hours of the beginning of attack, and the hand can be laid on something in the iliac fossa; if there is agonizing pain and tenderness and high temperature, it can be stated almost without exception that the case is not one of appendicitis. Catarrhal inflammation of the bowel will give a temperature that has never been seen in average cases of appendicitis. On the contrary, nine-tenths of cases of appendicitis have a normal or subnormal temperature, with inflammation at the head of the colon. The very expression of the face of the girl who had gangrenous appendicitis with high temperature, in the case alluded to, would have been sufficient to indicate to anyone with experience that she was suffering from acute sepsis and that her condition was one of very great gravity. When a mass cannot be demonstrated at the beginning, and if the attending physician says there was nothing there when the patient began to have pain, and that slowly the induration and mass had been growing, the diagnosis is made. If the mass is enlarging it is pus; if it is stationary it is not pus.

After reduction or untwisting of any strangulation, any injury to the lumen of the bowel, from constriction from any cause, there is left a condition that will require days of purgation before a natural movement of the bowels takes place. It is commonly a misnomer to speak of strangulation. The bowel is generally in a measure strangulated, but it should be so designated only when the bowel becomes gangrenous. Under these conditions recovery ensues in probably one per cent., or may be five per cent. of cases. In cases of obstructed hernia, however, there should be no mortality. There is no justification in operating for strangulated hernia because a man has to wear a truss, but the minute he gets it down and cannot get it back there exists an excuse to effect a radical cure and it should be availed of whenever possible.

In the treatment of the cases under consideration opium is to be absolutely avoided. This drug not only blinds the patient to his serious condition, but it blinds the doctor, who is made to feel that the patient is very much better, while the pulse may be rapid and symptoms of approaching death may be present. Purgation is a most useful means of treating all cases whether of impaction or of peritonitis.

Radical, perfect surgery cannot be done, if the bowels are gathered up into a lump and complete obstruction has taken place. Probably nine out of ten of the cases in which simply an incision is made and drainage is favored meet with success, because that is all that is required, but as a rule, it is necessary to thoroughly wash out the abscess, and separate every adhesion from the pelvis to the diaphragm in order to do a complete operation.

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